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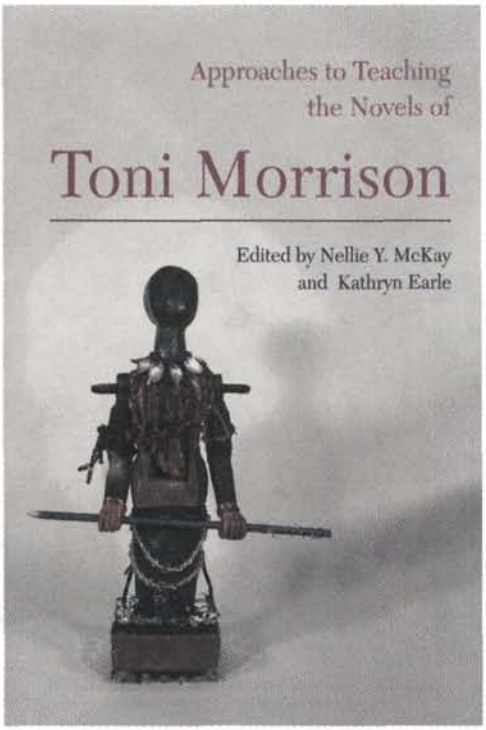

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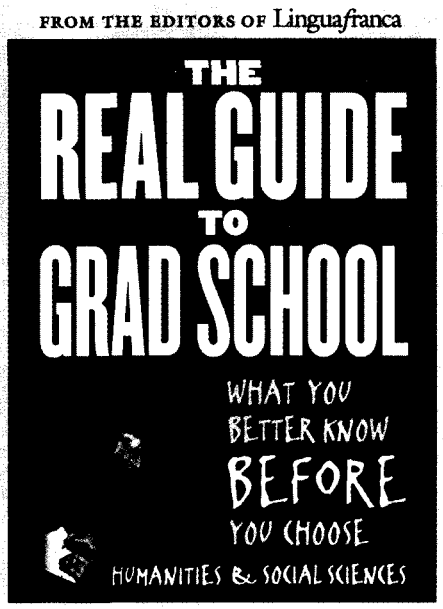

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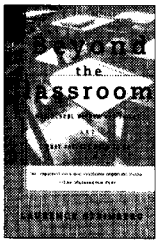

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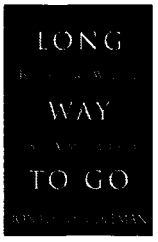

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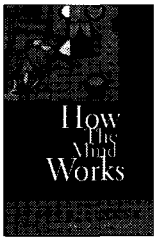

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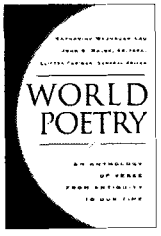

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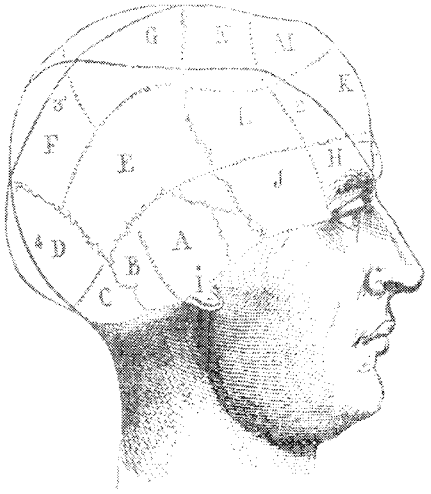

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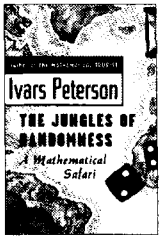

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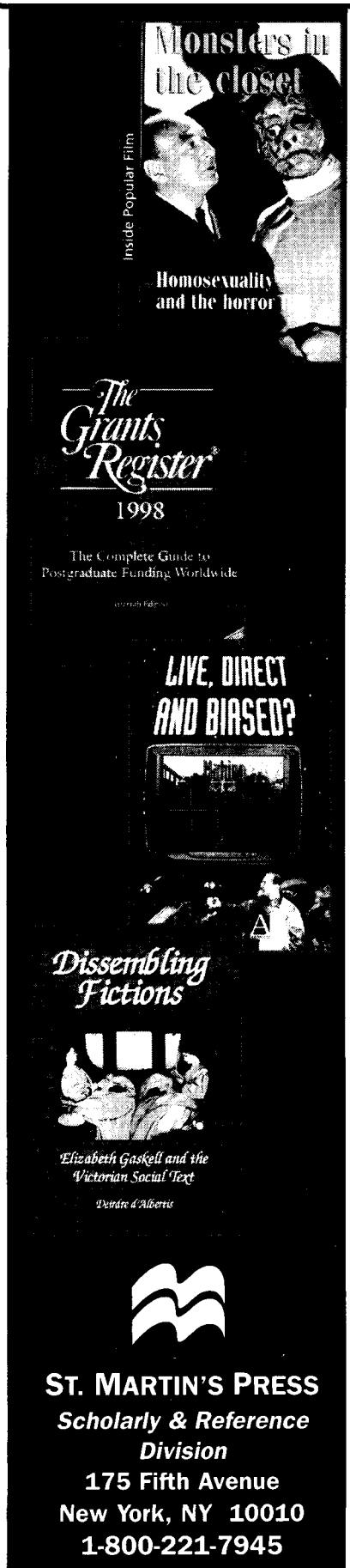

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AS NASTY AS THEY WANNA BE

Taking a look at today's academic novels is a fine idea, but Adam Begley's "The Decline of the Campus Novel" [*LF*, September] slants the evidence to fit its thesis. The academic novels steadily appearing through the 1980s and 1990s are just about as nasty (and enjoyable) as their predecessors. This includes those by Russo and Smiley, though Begley draws from an awfully narrow reading list. He should have looked at Robert Grudin's *Book*, Edward Allen's *Mustang Sally*, Cathleen Schine's *Rameau's Niece*, and Howard Jacobson's *Coming From Behind*, among many others.

Even Begley's quotations don't prove his point. When Russo proclaims that being promoted at West Central Pennsylvania University "was a little bit like being proclaimed the winner of a shit-eating contest," this is supposed to demonstrate a lack of satiric bite? Perhaps Begley means that the academics themselves are basically decent—in which case, why does Smiley make the central metaphor of her novel a bloated pig with an insatiable appetite?

As a fan of academic novels, as well as the author of one, I'd welcome some savvy analysis of this genre. Unfortunately, Begley's piece doesn't offer much.

DAVID GALEF
DEPARTMENT OF ENGLISH
UNIVERSITY OF MISSISSIPPI

Adam Begley's article is based on scarce evidence. For instance, he has considered only half of Richard Russo's novel—the comic half—and neglected the equally important issues of the serious half: the protagonist's abandonment by his self-important father, his rescue by his mother from a childhood suicide attempt, and his emotionally crippled adulthood. These developments contribute to the character's largely comic efforts to respond ethically, independently, and responsibly in a morally and intellectually bankrupt academic setting.

SUSAN HARRIS SMITH
DEPARTMENT OF ENGLISH
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CROSS WORDS

Chris Lehmann's interesting article ["Burning Down the House," *LF*, September] was marred by a serious error of fact. Lehmann asserts that "Times Books, once the upmarket nonfiction division of the Random House empire, has been dumbed down into a list now dominated by crossword puzzle collections." A quick glance at our

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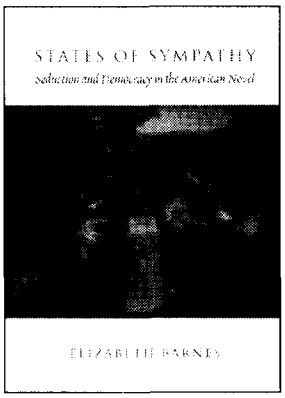

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Field *Notes*

LEISURE WORLD

IN 1990, WHEN JULIET SCHOR began researching how much time Americans spend working, it was hard to imagine a sleepier topic. Labor economists believed the question long settled: Economic growth and labor-saving technology were gradually ushering Americans into an arcadia of ever-increasing leisure. But Schor's work dealt a blow to that consensus. When *The Overworked American* (Basic) appeared in 1992, with its explosive finding that the average American was working a full month more a year than in 1969, Schor found herself the closest a Harvard economist can come to being a national sensation. Soon she was returning calls from *The New York Times* and *Newsweek*. Even the *Weekly World News* did a story. The book quickly became a national best-seller. The University of Chicago made it required reading in its undergraduate core curriculum.

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And then in May 1997, the Berkeley sociologist Arlie Russell Hochschild added her own contribution to the overwork hypothesis. Her book, *The Time Bind: When Work Becomes Home & Home Becomes Work* (Metropolitan), was promptly splashed on the covers of *Newsweek, The New York Times Magazine,* and *U.S. News and World Report*. (Hochschild found the latter's coverage so sensationalist that she professed to be "horrified" in a letter to the editor.) In academe, time is now anything but a sleepy subject—and, if these scholars are right, anything but available to the rest of us.

Unnoticed by the indignant overworked, however, was another book published at the same time as Hochschild's: *Time for Life: The Surprising Ways Americans Use Their Time* (Penn State), by University of Maryland sociologist John P. Robinson and Penn State professor of leisure studies Geoffrey Godbey. With considerable aplomb, it threatens to force the overwork thesis into retirement. The book begins with the authors' observation that "our counterintuitive findings have sometimes made us feel like the isolated boy who dared to point out that the emperor...was wearing no clothes." It goes on to declare that, contra Schor and Hochschild, the average American enjoys an hour *more* free time per day now than in the 1960s.

What gives? This is one cold war you can blame on détente. In graduate school at the University of Michigan in the early 1960s, co-author Robinson helped launch the first international social survey to go behind the Iron Curtain. Time use was chosen as the survey's focus; among other advantages, time was the most politically neutral subject anyone could think of. The next step was to devise the most reliable methodology for soliciting data. So the Michigan sociologists distributed "time diaries," in which subjects in twelve countries—including 1,244 individuals in the United States—recorded their every movement over the course of a single day in 1965. Thus began Robinson's long and productive career in time studies. In 1975, he administered an improved time-use survey to 2,406 Americans. Ten years later, he collected 5,358 more time diaries.

Over the years, time diaries have proved especially adept at revealing differences between how people spend their time and how they think they spend it. Most particularly, they suggest that people routinely overestimate the amount of time they devote to a given activity—work, for example. In fact, Robinson and Godbey claim, the time diaries show that in nearly every conceivable demographic group Americans have more leisure time on their hands than ever before. (Those with graduate educations, take note, are practically the only exceptions.) To bol-

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ster their case, they present some 31 tables and 19 appendices in the 367 dense pages of their book.

If Schor arrived at different results, it's partly because she used different data: Her numbers on working hours come from the Bureau of Labor Statistics, which usually relies on asking people how much they worked in the previous week—rather than on time cards or time diaries. Schor readily acknowledges that time diaries represent an improvement in certain respects over Labor Department data; she herself used Robinson's time diaries to estimate the hours spent by women on housework. "But with time diaries," she argues, "you're giving up a consistent representative sample over longer periods of time." She argues that the decline in work hours Robinson found between 1965 and 1975 is due to the biased character of his 1965 sample group, which, in order to match those of the other participating countries, restricted itself to urban families in which the head of the household was employed. As a result, she says, the sample was "whiter, more affluent, and more employed than the actual population." Schor points out that between 1975 and 1985, when Robinson's sample base was more representative, he found little change in the number of working hours. During this very period, adds Schor, "involuntary underemployment" was becoming an important structural feature in the U.S. economy, rendering the concept of more free time a cruel misnomer for many. "His whole story is about a ten-year period and a statistical artifact!" she cries in exasperation.

According to a new book, the average American now enjoys an hour *more* free time per day than in the 1960s.

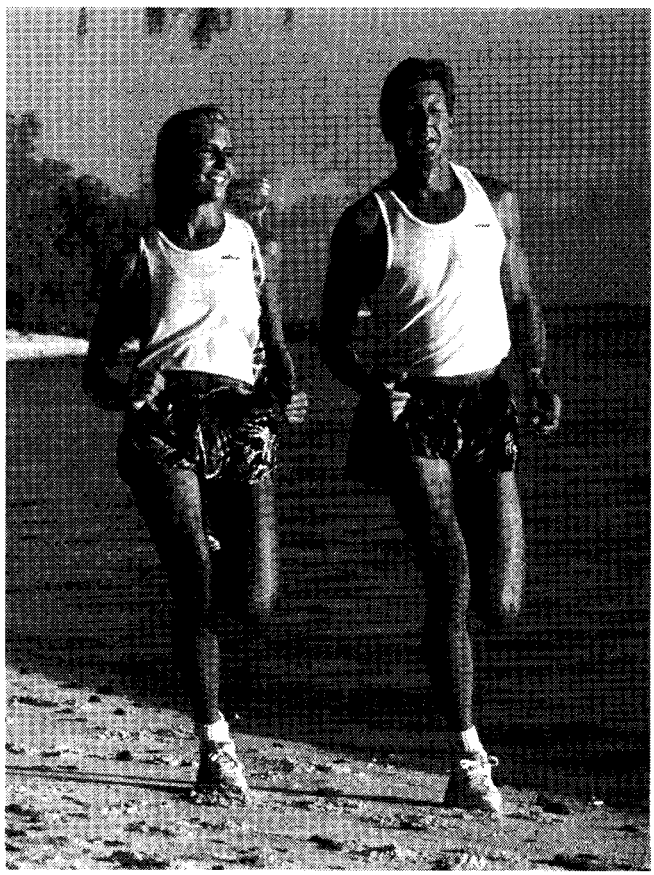
Unruffled, Robinson replies that the discrepancies between his earlier and later samples are more apparent than real—that, for example, there are no noticeable differences in leisure time between blacks and whites. He says he controls for underemployment to make sure that a lack of work is not misconstrued as leisure. And he adds, "We're beginning to get some stuff out on the 1990s data, and we find pretty much a continuation of trends that we found earlier."

Hochschild's central objection to *Time for Life* is simpler. "People who hang in for highly time-consuming time diaries," she says, are precisely those who have the most time on their hands—a sampling bias if there ever was one. But *Time for Life* counters with one of its most fascinating claims: Much the way the controversial *Sex in America* survey of a few years ago discovered that masturbation was more frequent among those who had plenty of sexual intercourse, Robinson and Godbey report that the more busy people are the more likely they are to take on additional activities.

Time for Life is littered with such ironies and paradoxes. Indeed, if its findings are correct, the volume should force sociologists to view time's qualities—it overlaps, it stretches, it contracts, it disappears—with the same wonder as theoretical physicists. It will charge them with explaining, for one thing, why Americans feel more rushed than ever before, even though their free time supposedly has increased.

So how do we know who's right? How do you factor the time taken out of a salaryman's ten-hour day for a quick jaunt to the corner newsstand or a leisurely lunch? Should the scholarly chrononaut make a statistical correction for downturns in the business cycle, when wageworkers are vulnerable to layoffs and decreased overtime? Schor gives the business cycle paramount importance in her calculations, while Robinson and Godbey claim it exerts little influence on overall time patterns. Schor counts the hours officially spent at work (9 to 5, for example), while the time diarists have no problem counting gossip sessions around the watercooler as leisure time. Ordinary Americans may be finding time for work, or they may be finding time for leisure. We won't know which until sociologists agree on how to find time in the first place.

RICK PERLSTEIN



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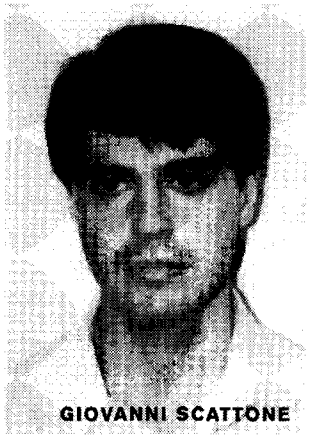
FLORAL WREATH FOR MARTA RUSSO

[illegible]

FROM THE ASSASSINATION of Julius Caesar to that of Aldo Moro, Romans have long been used to seeing their celebrity residents come to violent ends. In the case of Marta Russo, however, it was her murder that made her famous, transforming her over the course of the summer from an unknown twenty-two-year-old law student into the apparent victim of what the police are calling *un delitto filosofico,* or philosophical crime. On the morning of May 9, Russo was shot in the head while walking with a friend across campus at the University of Rome. She died four days later, prompting a Roman orgy of public grieving, torch-lit parades, and all-night vigils. Italy's prime minister, along with thousands of students, attended Russo's funeral—which was broadcast on national TV. In the meantime, without a murder weapon or a suspect, the investigators' only lead in the case was the trajectory of the fatal bullet—which they traced to a window in the Institute of Legal Philosophy at the university's law school.

When no one at the school would talk, investigators suspected a cover-up—a theory that was picked up by the press, who, invoking the Mafia's infamous code of silence, accused the school of *omertà*-like behavior. [illegible] gained credence when a wiretap caught the director of the institute, Bruno Romano, ordering his staff not to cooperate with the inquiry. Romano, a scholar of Heidegger and Lacan, spent more than a week under house arrest, an event the press ascribed largely to his off-putting erudition. "I've never known anything, nor did I pressure anyone," he told *La Stampa* in an interview from his home on June 20. "But the modalities of evil are ambiguous; evil insinuates itself." When asked why he thought he had been detained, Romano replied, "Well, words can have many interpretations. The truth is always linked to interpretations. It is the lan-[illegible] of the enigmatic. It is not a mathematical formula, and everyone receives truth in their own way."

MARTA RUSSO



GIOVANNI SCATTONE

Romano's cryptic comments only added to the intrigue surrounding the murder, and, simultaneously, renewed a national debate about the university system's need for reform. In an apparent attempt to rebuff mounting criticism, the university's rector ordered a campuswide internal investigation. It was long overdue. The law school, which has just eighty professors to teach its 40,000 students, has a particularly dubious reputation. Reports of professors who show up on campus only on payday and who regularly farm out their lectures and grading to graduate students are legion. Nepotism and bribery are not uncommon; the president of the law school told a local newspaper in June that even he has been approached by students offering him favors in return for grades.

Italy's university system has long been inefficient and unwieldy, a fact some professors say makes corruption inevitable—even necessary. In the 1960s, democratically minded reformers opened the

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Italian police investigate what they call a "philosophical crime."

universities to everyone without expanding or updating facilities to accommodate the surge in enrollments. The result is extreme overcrowding. The University of Rome, for example, was originally designed for 15,000 students but currently has about 180,000 enrolled and possibly 20,000 more who attend off the books. (Not surprisingly, the dropout rate in some programs is as high as 70 percent.) The school's bureaucratic rules and procedures have not been updated either; they are based on the medieval idea of a university as an autocratic, self-policing entity. "It's a modern institution pretending things are in the fifteenth century," says one professor. "Like a number of institutions in this country, the only way the system can operate is to tolerate a degree of irregularity," says another. "You have to work around the rules to make it work."

The combination of total silence, pompous philosophizing, and whiffs of corruption emanating from the university this summer only exacerbated the general perception of Italian academe as a kind of lawless fiefdom. Finally, a month after the killing, a secretary at the law school broke the silence surrounding the case, confessing to police that she had been in Room 6 of the institute on the afternoon Russo was shot. She reported seeing two graduate students, twenty-nine-year-old Giovanni Scattone and thirty-year-old Salvatore Ferraro, standing near one of the windows. As she turned away, she heard a blast—like that caused by gunfire. Looking back, she saw Scattone drop a pistol into a bag. Immediately, she said, the two suspects fled the premises.

Scattone and Ferraro were arrested, charged with voluntary manslaughter, and locked into a single cell, where they continue to languish, adamantly maintaining their innocence. Recently, however, the police made what they consider an important break in the case when they turned up a seventy-five-page document written by the suspects and sold to students on campus for thirty dollars a copy. In it, Scattone and Ferraro, both star students of the philosophy of law, and occasional lecturers for a course on legal logic, describe the perfect crime. In order to solve a crime, they write, making reference to learned arguments on juridical evidence and proof, three elements must all be known: place, motive, and weapon. Without such knowledge, they insist, proving responsibility for a crime is impossible. So, were Scattone and Ferraro engaging in a perverted form of that time-honored academic objective: putting theory into practice? That's the current explanation being offered by Roman investigators, who speculate that the two legal theorists never meant to kill anybody but just wanted to fire a gun, cover their traces, and see what would happen.

Of course, the accused aren't buying this theory, preferring a defense founded on more empirical data. Scattone's lawyer asks: "How can a man no taller than five feet six shoot out of a window that is five feet off the ground and is obstructed on one side by an old air conditioner?"

JAY FIELDEN

MOSTLY MOZART

THIS PAST SUMMER, UNIversity of Northern Iowa musicologist David Buch stumbled upon a series of Mozart opera fragments in a Hamburg archive. The music—songs from a collaborative series of fairy-tale operas—was hailed as a welcome (if whimsical) addition to the composer's corpus. *The New York Times* trumpeted the good news on the front page. But David Cope, a composer and professor of music at UC-Santa Cruz, has done Buch one better: He recently unveiled Mozart's hitherto unknown 42nd Symphony. The thing is, Cope didn't recover a lost score. He whipped up a new one on his computer.

The 42nd bears the distinct aural imprint of late Mozart symphonies like the *Jupiter*. That's because Cope's creation is a sophisticated pastiche, generated by feeding his computer thousands of choice bits from the forty-one genuine symphonies—with an emphasis on musical strands from the later, more mature works. Cope's computer, EMI (Experiments in Musical Intelligence), parses musical scores as a succession of tiny "events"—a percussive phrase, a melodic fragment—that form a sort of musical grammar. These events are then classified into entities like "antecedents" and "consequents," which identify how each musical moment functions in context. As a result, when the computer tweaks, modulates, and blends the fragments together, the finished product remains stylistically sound: A concluding cymbal crash (a "consequent") doesn't just pop up in the second measure of a lilting andante movement, for example. Cope's grammar also allows a symphony's musical theme (its "statement") to be developed throughout the piece. Most importantly, Mozart's voice doesn't suddenly scramble into Schoenberg's.

Cope also uses EMI to ferret out a composer's "signatures," or habitual flourishes. With the Mozart symphonies, for example, EMI detected about forty recurrent gestures, from favored rhythms to

Cope didn't uncover a lost Mozart symphony. He whipped up a new one on his computer.

W. A. MOZART

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modes of orchestration. (The approach is analogous to the computer analyses of Vassar Shakespearean Donald Foster, who recently tapped Joe Klein as the author of *Primary Colors* based on his use of signature phrases like "slimetudinous" and "mega-explain mode.") While mainstream musicologists "tend to dismiss such patterns as clichés," Cope says, he sees them as "proud markers of a composer's style." By salting the computer's database with these signatures, Cope ends up with something that he claims sounds eerily like the real thing. Speaking of EMI's Mozart symphony, Cope grants that it is "not quite genius" but boasts that it is "pretty terrific, and at least as good as Salieri."

Silicon-savvy musicologists have toyed around with computers for years, but usually with tepid results. It's not too hard, for example, to get a computer to create a fake Bach chorale by teaching it the basic rules Baroque composers followed. (This is what Music Theory 101 forces students to do every semester, albeit on paper.) Rather than learning the specific rules of a musical form, Cope's computer can mimic all types of musical idioms: The same algorithms can yield mock Bach and imitation intermezzi. By feeding in the scores of different composers during a single data dump, Cope creates Dr. Moreau–like musical hybrids—for example, a cross of Igor Stravinsky and Charles Ives. He's even taught EMI how to compose in the style of...David Cope. "It helps me brainstorm," he says.

The EMI system is actually based on a very old musical form: the *musikalisches Würfelspiel*, or musical dice game. All the rage in eighteenth-century parlors, these entertainments presented a sequence of numbered musical fragments that could be reassembled in myriad ways. A toss of the dice determined the musical path. (A typical *Würfelspiel* of sixteen measures yields billions of possibilities.) Although music theorists typically present such creations—penned by eminences like Haydn and Mozart—as mere curios, Cope feels they yield genuine insight into the composing process. "The genius of great composers," Cope likes to say, "lies not in inventing previously unimagined music but in their ability to effectively reorder and refine what already exists."

Of course, most composers recoil at such a description of their work. And yet Cope insists it's no insult. After all, we don't sneer at recombinant DNA, so why sneer at recombinant music? "Composers cling to this idea of inspiration, but all music is woven from old tapestries," he says. In fact, Cope first embarked on the EMI project out of a desire to dust for his own musical fingerprints. "I'm very interested in someone being able to recognize my music as Cope. Of course, John Cage was thrilled that he often couldn't recognize his own scores!"

According to Cope, many experts fail to recognize the difference between EMI's compositions and authentic works. He claims to have performed "blindfold tests" on fellow musicologists and fooled "the vast majority of them." (He can't say who, he quickly adds, because "they would kill me.") EMI's success may

We don't sneer at recombinant DNA, so why sneer at recombinant music?

depend on the fact that its creations often sound like *very* close variations on extant themes: One fake Schumann piece, for example, heavily echoes the opening episode of *Kinderszenen*. EMI's melody is simply less—how to put it?—inspired. The computer does have its champions, however: Artificial-intelligence expert Douglas Hofstadter, an amateur pianist, says he was "stunned" by the authenticity of EMI's take on the Chopin mazurka. EMI's critics are equally passionate. The L.A. *Times* editorial page recently likened EMI to "a visionless Hollywood producer who reassembles the patterns and sequences of a cinema masterpiece."

Of course, it's hard to listen to EMI's compositions without prejudice. Although Cope claims to have "wept" at a recent performance of an EMI Rachmaninoff piano concerto, for most music lovers EMI's creations seem mainly to evoke indignation. At the premiere of the 42nd Symphony this past spring at Santa Cruz—performed using period instruments by Mozart specialists, in order to give EMI an extra boost—the audience was filled with traditionalists prone to harangues about the soulless computer. A few artificial-intelligence cheerleaders offered weak counterpoint. Purely aesthetic responses seemed all but impossible.

Perhaps that's why Cope hasn't been able to publish a single score—not even his own—since taking up with EMI. "The publishing companies simply refuse to consider them!" he thunders.

Cope doesn't plan to stop now, however. His next big project: an opera called *Mahler*. "Gustav Mahler was a great opera conductor," Cope explains, "but he never wrote one himself. So I'm using his symphonies to generate a grand-scale opera, complete with two choirs." The libretto will be based on the composer's letters. Cope, who is partially motivated "by the fact that I'm bored with my record collection," estimates he will need about two years to enter the data from previous Mahler scores. But that's the hard part. Once he's done, EMI can spit out a complete aria in about twenty minutes.

DANIEL ZALEWSKI

TWO VERSIONS OF A MOZART SIGNATURE

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IN DUBIOUS BATTLE

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IN THE HEARTS OF AMERica's reading public, John Steinbeck occupies a certain pride of place. *The Grapes of Wrath,* to cite just one of his novels, sells more than ten thousand copies a year in the popular Signet edition alone. But popularity with the public doesn't necessarily translate into academic interest, as Florian J. Shasky discovered. The California-based bookseller has spent the last seven years trying—and failing—to sell a large stash of Steinbeck letters to one of America's premier research universities. Robert DeMott, an eminent Steinbeck scholar at Ohio University, has vetted the collection and deemed it "groundbreaking." And last year, Shasky cut his asking price by 30 percent—to $1.3 million. But today he's still waiting for academe to call.

In 1989, when the descendants of Steinbeck's sister Esther Rodgers asked Shasky to help sell the letters, he readily agreed. As a former chief of special collections at Stanford University, Shasky had been the custodian of one of the country's most consider- [illegible] There are [illegible] (similar size.) Today, even with no potential offers in sight, he still believes the Rodgers Collection is worth every penny of its orig- [illegible] Besides offering first editions of Steinbeck's novels, inscribed copies of many of his books, and hundreds of photographs of the Steinbeck family, the collec-

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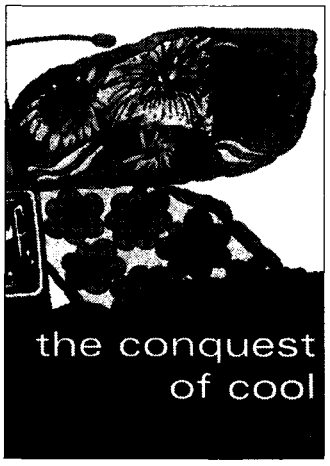

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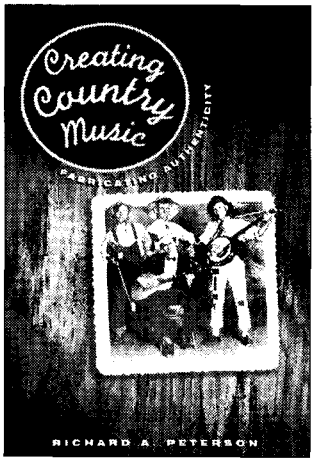

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tion contains letters spanning four generations of the Steinbeck family, including more than two hundred from the writer to his sister. One of these may be the only signed letter Steinbeck ever wrote in Spanish; among other things, says Shasky, this letter proves the writer's fluency in that language: "People know that Steinbeck could swear at fishermen in Spanish. But this is a *well-written* letter." There is also Steinbeck's earliest surviving juvenilia, an undated poem he gave to his father that contains the delightfully odd line, "Steakus Steakus, Lord of Meat."

According to DeMott, the collection debunks the widely held notion that Steinbeck was a rugged individualist who severed all ties to his family. "The letters show Steinbeck's continual engagement with his parents," he says. "Even when Steinbeck was supposedly in exile writing his first novel at Lake Tahoe, he received care packages from his family and got upset if he couldn't mail them a letter." But despite the scholarly endorsement, Shasky struck out at an extraordinary array of institutions, including the Center for Steinbeck Studies at San Jose State University, the Pierpont Morgan Library, the Library of Congress, and the Harry Ransom Humanities Research Center at the University of Texas at Austin, as well as Columbia University, and Stanford, his former home roost.

When Steinbeck won a Nobel Prize, *The New York Times* attacked the jury's selection.

Asked why she passed on the Rodgers Collection, Susan Shillinglaw, director of the Steinbeck center at San Jose State, said the collection was too expensive and its contents not all that revealing. Leslie Morris, the curator of manuscripts at Harvard's Houghton Library, was not offered the collection; but she explains that, like a lot of libraries now, Harvard generally favors collections that appeal to a broad range of scholarly interests over strictly biographical material. "When we assess a collection," she says, "we ask what other research it can support and what's going on in the various departments at the university to support it."

To Shasky, carefully worded statements such as these are merely cover for rampant anti-Steinbeck sentiment in the academic community. During Steinbeck's lifetime, the New York literary establishment dismissed the novelist as a middlebrow, regional writer and condemned him for his popularity. When he won the Nobel Prize, in 1962, *The New York Times* ran an editorial criticizing the jury's selection of a writer whose major work was already two decades old. "It raises questions about the mechanics of selection and how close the Nobel committee is to the main currents of American writing," the paper opined. Now, avows Shasky, Steinbeck is being shunned by scholars because "he supposedly demeans women."

DeMott believes the problem is Steinbeck's conventional prose style: "He's not formally experimental and doesn't fit into the modernist canon," he says sadly. "He certainly

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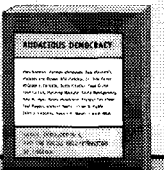

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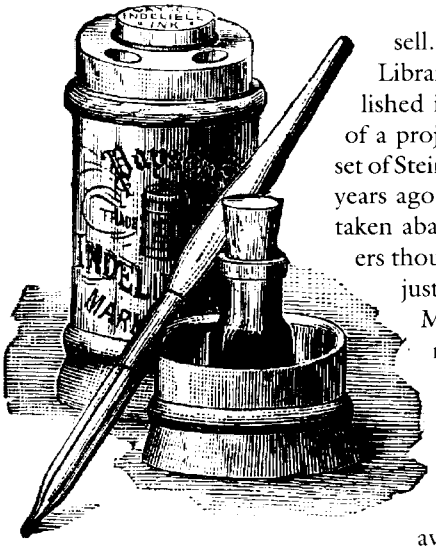

isn't taught in the academy today." James Lowe, an autograph expert and consultant to R.M. Smythe, an auction house in New York City, speculates that Steinbeck will never fetch a high price because he's now considered "a second-tier writer"—and anyway, letters of a personal nature don't sell. Indeed, when the Library of America published its first installment of a projected four-volume set of Steinbeck's works three years ago, a few critics were taken aback. "Some reviewers thought that Steinbeck just didn't belong with Melville," says Library of America publisher Max Rudin, who notes with satisfaction that the two Steinbeck volumes currently available are among the series' best-sellers.

It's possible that anti-Steinbeck sentiment explains why Shasky was unable to find a patron willing to buy the collection and donate it to a university as a tax write-off. (Not for lack of trying though; he says he approached members of the Golden State elite like Barbara Boxer, Dianne Feinstein, and Leon Panetta.) Then again, Shasky's prices may be out of line for the material he has to offer, and Steinbeck is hardly the only writer of his generation whose reputation has waned. A substantial collection of Faulkner screenplays lingered on the market for four years, and a set of Hemingway letters failed to excite a single bid at New York auctions last winter. Such downturns are not restricted to modernists. When *The Forsyte Saga* was a must-read in the early part of this century, John Galsworthy material was all the rage, says Lowe. Now, he says, "you couldn't sell a Galsworthy letter to a high school library."

Nevertheless, Shasky remains incensed by his experience with Stanford, explaining how, several years ago, when he tried for the third time to place the Rodgers Collection there, the university offered him a "stunningly low" $250,000 and then turned around and snapped up Allen Ginsberg's entire personal archives—1,500 linear feet of material—for $1 million. "Why?" asks Shasky in despair. "The Beats are hip."

He has now abandoned his attempts to place the Rodgers Collection in the academy altogether. But his remaining options aren't particularly appealing. On the one hand, he says, "If I sell the collection to Sotheby's, they'll break it up." On the other hand, he'd at least be able to unload most of it. "The Japanese are madly interested," Shasky notes, as are "certain corporations in Silicon Valley."

RACHEL SHTEIR

Canto

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Foreword by ***Maxine Berg***

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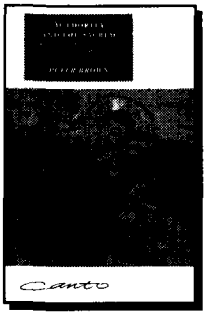

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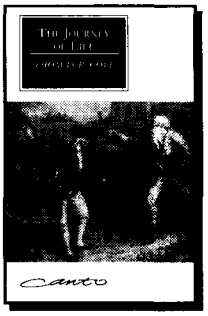

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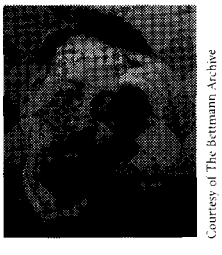

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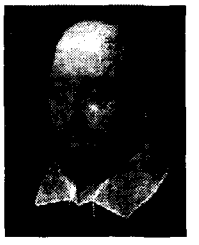

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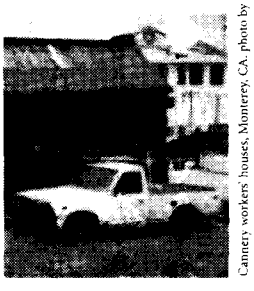

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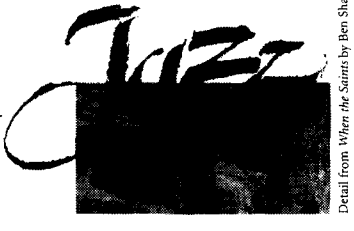

Detail from *When the Saints* by Ben Shahn. Courtesy Bettmann Archive.

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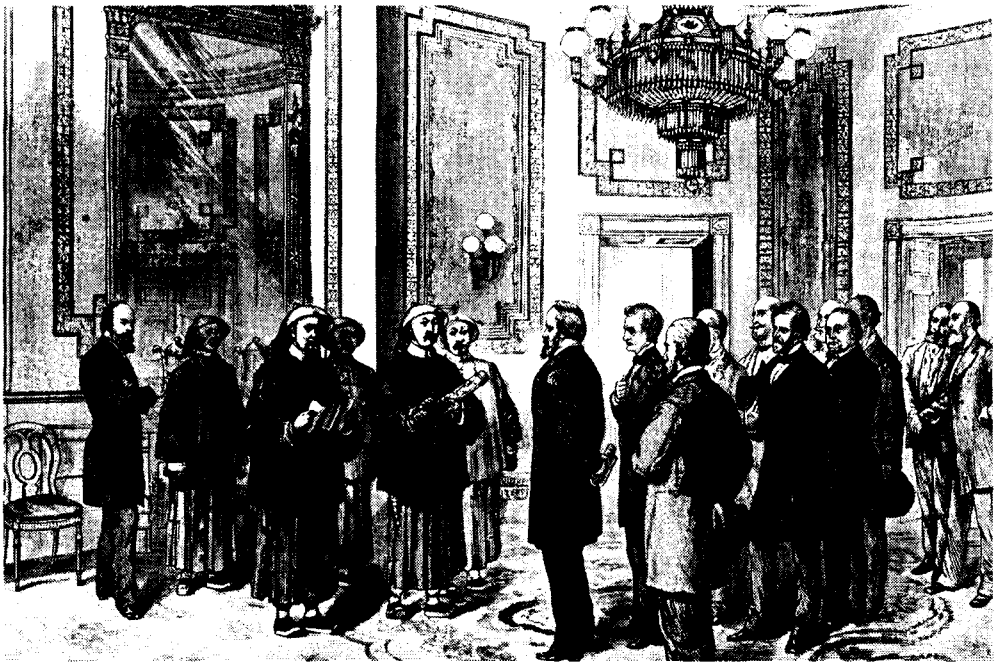

Miriam Fendius Elman, assistant professor of political science at Arizona State University and editor of *Paths to Peace: Is Democracy the Answer?* (MIT, 1997).

"Since the end of the Cold War, nothing has generated more controversy in the field of international relations than the assertion that democracies have rarely gone to war with each other," Elman observes. "As the United States searches for a new foreign policy blueprint, expanding the zone of democracy abroad seems to fit the bill. Two of the best introductions to the burgeoning 'democratic peace' literature are **Bruce Russett's *Grasping the Democratic Peace: Principles for a Post–Cold War World*** (Princeton, 1993) and **James Lee Ray's *Democracy and International Conflict: An Evaluation of the Democratic Peace Proposition*** (South Carolina, 1995). Russett and Ray each offer a host of reasons why democracies tend not to fight each other, from internal checks and balances that prevent leaders from waging war to liberal values that foster peaceful methods of conflict resolution. And in doing so, these books largely discredit the traditional 'realist' approach to the study of world politics—a perspective that minimized the impact of domestic politics and political ideology on war and peace decision making."

....

Walter Laqueur, co-chairman of the International Research Council of the Center for Strategic and International Studies in Washington, D.C., co-editor of the journal *Contemporary History,* and author of *Fascism: Past, Present, and Future* (Oxford, 1996).

"Religious fundamentalism has played a notable role in international relations in recent years. One of the most enlightening contributions on the subject is ***Spokesmen for the Despised: Fundamentalist Leaders in the Middle East*** (Chicago, 1997), edited by **R. Scott Appleby.** The essays on diverse religious figures are uniformly excellent, the subject is of tremendous topical importance, not least because there is a great deal of ignorance about it to overcome. Even in Arabic and Persian not much has been published, though the sermons of the most popular preachers can be bought on cassette or CD on street corners."

....

Cynthia Enloe, professor of government at Clark University and author of *Bananas, Beaches, and Bases: Making Feminist Sense of International Politics* (California, 1990) and *The Morning After: Sexual Politics at the End of the Cold War* (California, 1993).

"The last seven years have seen a flourishing of feminist studies challenging the alleged gender neutrality of the 'national interest.' Earlier work examined Filipino and Mexican maids as serious international political actors, for example, and the role of rape in wartime. Now we have the first detailed feminist scholarly case study of a military alliance. **Katherine Moon's** new book, ***Sex Among Allies: Military Prostitution in U.S.–Korean Relations*** (Columbia, 1997), reveals just how central our history of providing Korean women as prostitutes to male American soldiers has been in negotiations between Washington and Seoul. Drawing on declassified diplomatic minutes and her own interviews with Korean women working around U.S. bases, Moon has given us an eye-opening account of a little-known subject."

....

Walter LaFeber, professor of history at Cornell University and author of *The Clash: U.S.–Japanese Relations Through History* (Norton, 1997).

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analyzed from a Soviet–U.S. perspective," says LaFeber. "But **Thomas J. McCormick's** ***America's Half-Century: United States Foreign Policy in the Cold War and After*** (Johns Hopkins, 1995) broadens the focus so we can understand how that relationship and the United States and Soviet Union's internal development were shaped by other nations in an increasingly interdependent world. McCormick is the first to apply world-systems theory comprehensively to the Cold War. That conflict, he demonstrates, turned on complex 'center-periphery' relationships, with certain 'peripheries'—most notably Vietnam and oil-producing nations—causing fundamental problems for the United States and for the American triumphalist view that underlies so many other books on the Cold War."

Michael Doyle, director of the Center of International Studies at Princeton, author of *Ways of War and Peace: Realism, Liberalism, and Socialism* (Norton, 1997), and co-editor of *Keeping the Peace: Multidimensional U.N. Operations in Cambodia and El Salvador* (Cambridge, 1997).

"Keeping the peace in countries riven with factional conflict, protecting basic human values against predatory leaders and murderous animosities—these are among today's greatest moral and strategic challenges. Nowhere are they better illustrated than in two books on the July 1995 massacre at Srebrenica in Bosnia. **David Rohde's** ***End Game*** (Farrar, Straus & Giroux,

1997) recounts, hour by hour and in painstaking detail, the killings in those Bosnian hills as Serbs hunted their Muslim former neighbors. **Jan Honig** and **Norbert Both,** in ***Srebrenica: Record of a War Crime*** (Penguin, 1996), take a more panoramic view. Their book focuses less on who did what than on how the United States, NATO, the United Nations, and well-meaning, responsible states—the Netherlands, most importantly—agreed to accept the dangerous mandate to safeguard the area and why they failed to fulfill it. Ensuring real peace in Bosnia today requires that we learn how to prevent another such massacre. The first step is learning from Srebrenica, and these two books are an excellent place to start."

John Mearsheimer, professor of political science at the University of Chicago and author of *Conventional Deterrence* (Cornell, 1983) and *Liddell Hart and the Weight of History* (Cornell, 1988).

"It is frequently asserted that conquest does not pay in the modern world because modern industrial economies are

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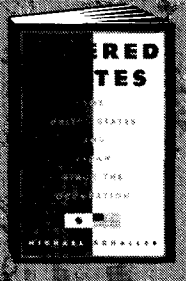

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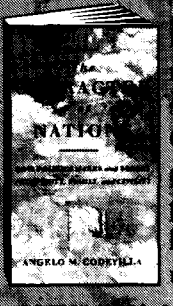

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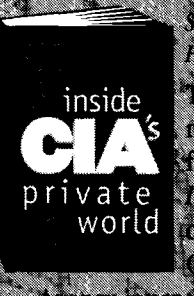

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ern industrial economies are especially difficult to exploit for gain. But in ***Does Conquest Pay? The Exploitation of Occupied Industrial Societies*** (Princeton, 1996), **Peter Liberman** carefully assesses the historical record to show that this assertion is not true. Indeed, he makes a persuasive case that modernization has made great powers easier, not harder, to plunder. If conquest pays, great-power war might be a more attractive option than is commonly believed. **Robert A. Pape's *Bombing to Win: Airpower and Coercion in War*** (Cornell, 1996) also cuts against the grain, this time challenging the conventional wisdom of airpower advocates, who argue that massive air strikes on an enemy's civilian population will quickly induce surrender. Pape systematically examines the history of strategic bombing and shows that it has little military utility, mainly because modern societies can absorb tremendous amounts of punishment without flinching."

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Michael Lind, contributing editor of *Harper's Magazine*, author of *The Alamo: An Epic* (Houghton Mifflin, 1997) and *The Next American Nation: The New Nationalism and the Fourth American Revolution* (The Free Press, 1995).

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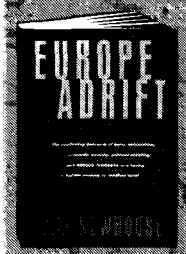

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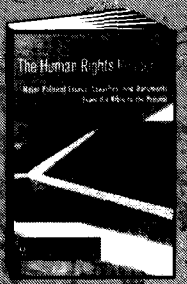

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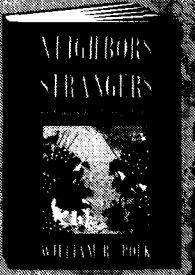

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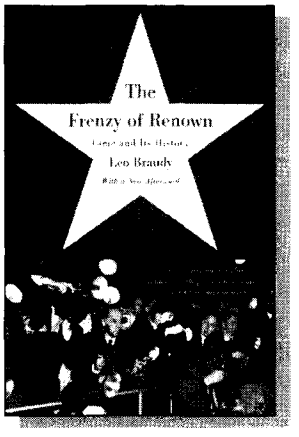

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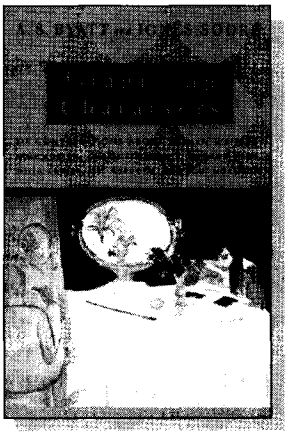

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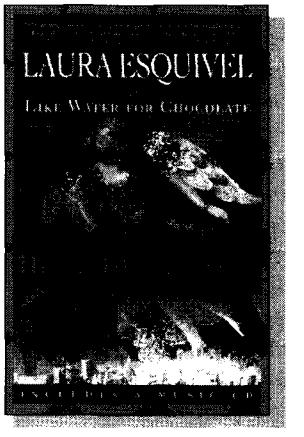

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Inside*Publishing*

THE BOOKS FROM IPANEMA

OXFORD HAS FOUND GOLD IN LATIN AMERICA! So begins an emphatic letter sent out by Oxford University Press to reviewers, alerting them to a new series of classic Latin American texts that it is launching this fall. To hear Oxford tell it, every book south of the Mexican border was slowly crumbling to dust until the white-knight publisher came along. The result is a purported boon not just to Argentines, Chileans, and Brazilians—who had neglected their heritage to such a degree that it was "quite literally rotting on the shelves"—but to restless U.S. readers ready for something new. According to Oxford, the series is a literary event equivalent to what would happen "if Pushkin, or Chekhov, or Rousseau, or Goethe, or Flaubert were being translated for the first time into English."

The series's general editor, Jean Franco, professor emerita at Columbia University, is not so comfortable with these grand claims. "Oh, that's ridiculous!" she said when she learned how the Library of Latin America is being presented. "That's horrendous! I'm going to have to phone them and tell them to stop it." Franco said that she (along with Richard Graham of the University of Texas at Austin, who handled translations of Brazilian texts) had more modest goals. First, the project brought academics together across national boundaries: Most of the new translations will be accompanied by essays from a U.S. specialist and a counterpart in the country where the book was written. Second, they wanted to correct the impression among U.S. readers that Latin American literature sprang up out of nowhere in the 1960s.

Americans who read books are familiar with Borges and Neruda and García Márquez, for instance, but they tend not to know about Joaquim Maria Machado de Assis, a mulatto grandson of slaves (he was also myopic, epileptic, and an orphan) who wrote some of the most deliriously adventurous fiction of the nineteenth century. Machado has appeared in English before, and Susan Sontag, among others, has set him a place at the table of great writers. But he has yet to grab a wide U.S. readership. The Library of Latin America will give us four chances to see what we've been missing, beginning with *The Posthumous Memoirs of Brás Cubas,* a novel (beautifully translated by Gregory Rabassa) that is told from the point of view of a wealthy—and, when the story begins, already dead—bachelor. Among its many witty twists are a brief chapter called "Transition," which commands the reader to "watch the skill, the art with which I make the greatest transition in this book," and a chapter called "The Old Dialogue of Adam and Eve," which contains no words—only ellipses, question marks, and exclamation points. Machado's novel also parodies the bourgeois French culture that so influenced nineteenth-century Rio de Janeiro and compares itself favorably to great classics of world literature, including the Bible.

Machado is less a precursor of magic realism than proof that Latin America produced playful, imaginative fiction long before Gabriel García Márquez began chronicling the town of Macondo, with its residents who sprout tails and ascend to heaven. Still, much of the nineteenth-century Latin American canon consists of nonfiction—by essayists, philosophers, and historians trying to give shape to young and fragile nations. One of the first such books that Oxford will publish is the selected writings of the Venezuelan Andrés Bello, a friend and tutor to Simon Bolívar, who was exiled in London and ended up in Chile, where he wrote the civil code, founded the university, and campaigned for national literacy. Bello was a polymath who wrote on such scattered topics as Roman law and the history of the alphabet. Another important nonfiction work is *Chapters in Brazil's Colonial History* by João Capistrano de Abreu. According to the introduction, Abreu's thinking on the settling of the

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Brazilian frontier is comparable to that of the American Frederick Jackson Turner, and his identification of racial archetypes—the "taciturn" Portuguese, the "melancholy" Indian, and the "good-natured" African—were incredibly influential. On the downside, Abreu was apparently a little lazy, a chain-smoker who talked more than he wrote, and did his best historical musing while reclining in a hammock.

Such books may interest a narrower audience of academic specialists. But the Oxford series can afford to be eclectic, because the project isn't strictly a moneymaking venture. A good deal of the funding has been supplied by a mysterious, low-profile organization called the Lampadia Foundation, which usually works in South America but is incorporated in Liechtenstein. Its money comes from a tin-mining fortune made by a handful of industrious Central European Jewish refugees; in the words of a rather floridly written Lampadia report, the men typically landed in South America carrying nothing more than "a suitcase of imitation leather that contained a clean collar, a change of underwear, and a set of Goethe bound in genuine morocco." In fact, the entire Oxford series is the brainchild of the current president of the Lampadia Foundation, an intriguing (and, when asked about the project, rather grumpy) figure named Robert Glynn, who is not an editor or professor or translator but a partner in the Manhattan law firm of Becker, Glynn, Melamed, and Muffly.

Machado de Assis wrote some of the most deliriously adventurous fiction of the last century.

Where did Glynn get the idea for the Library of Latin America? He says he was talking to a Brazilian friend about the number of books that had helped Brazil to form a sense of itself but were now out of print. He got to thinking that there should be some kind of library, a mix of greatest hits, like Machado, with nonfiction works that may not be on the tip of everyone's tongue but are still decisive in the history of Latin American thought—something along the lines of the U.S.'s Library of America. He got the Andrew Mellon Foundation to pitch in and decided to go with English translations first, because U.S. publishers were better equipped to put out lasting, high-quality editions. But he says that down the line Oxford promises to translate Spanish originals into Portuguese, and vice versa.

Talking to Glynn, one gets the sense that he considers introducing U.S. readers to Latin American literature a fine goal but that English-speaking readers are not as high a priority for him as South American ones. Alas, one also gets the sense that, outside of the specialists who worked directly on the project, Glynn has not been overly impressed by American academia, with its unfortunate combination of arrogance and a desperate need for financial support. Lampadia's flexible charter allows it to skirt the rules that govern many foundations: For instance, while the Andrew Mellon Foundation only gives money to institutions, Lampadia can help out individuals. And as word spreads about the Library of Latin America, Glynn dreads the prospect of both individuals and institutions lining up outside his door. "I don't need the publicity," he says. "Don't you know the way universities work? They touch it, they steal some of the money. Universities are awful."

SARAH KERR

CHUNNEL VISION

IN HIS NEW COLLECTION OF ESSAYS, *ON HISTORY* (New Press), Eric Hobsbawm writes: "Much of my life, probably most of my conscious life, was devoted to a hope which has been plainly disappointed, and to a cause which has plainly failed: the communism initiated by the October Revolution. But there is nothing which can sharpen the historian's mind like defeat." If the astonishing success of *The Age of Extremes*, the British Marxist's chronicle of the twentieth century, is any indication, much of the world's reading public heartily agrees. Translated into twenty languages and acclaimed by reviewers of virtually every political stripe, *The Age of Extremes* was voted one of the ten best books of 1996 in dependably anticommunist Taiwan and has recently gone into its fifth printing in Germany. Next year, the citizens of formerly communist Albania will have an opportunity to read Hobsbawm in a translation subsidized by the billionaire financier George Soros.

There is, however, one country that has all but declared *The Age of Extremes* unpublishable: France. Not a single French publisher—not even Fayard, Hobsbawm's longtime imprint—wants to translate his book. Since *The Age of Extremes* has proved itself an instant classic elsewhere, the absence of a French translation has caused a fair amount of consternation among French intellectuals. This

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past January, Pierre Nora, one of France's most powerful editors, issued a public explanation for his decision not to go ahead with the book, while *Le Monde Diplomatique,* the left's flagship journal, accused French publishers of McCarthyism—an ironic choice of pejorative in view of the warm welcome the book received in the States.

While it's true that the French are notoriously slow to discover scholarship in other languages—E.P. Thompson's *The Making of the English Working Class* did not meet their exalted standards for decades—the rejection of *The Age of Extremes* is a rather different matter. Hobsbawm has long been an honored figure on the French intellectual scene. Most of his books are translated, and *Nations and Nationalism* was recently placed on the *programme d'agrégation* for college students. Moreover, the importance of *The Age of Extremes* has not gone unacknowledged in France: This past January, *Le Débat,* the renowned intellectual journal edited by Nora, featured a nearly hundred-page symposium on Hobsbawm's interpretation of the twentieth century. "Twenty-five years ago," observes Tony Judt, an historian at NYU, "*The Age of Extremes* would have been translated in a week." So what's changed? Three forces have apparently conspired to keep the book out of translation: the growth of a vituperative anti-Marxism among French intellectuals; a budget squeeze in humanities publishing; and, not least, a publishing community either unwilling or afraid to defy these trends.

Though the French may view *The Age of Extremes* as an apologia for Soviet communism, this ironic, detached work has little to recommend it to propagandists pining for a return to the Iron Curtain. A member of the British Communist Party since his days at Cambridge in the late 1930s, Hobsbawm continues to consider the Bolshevik triumph in 1917 a genuine social revolution rather than a putsch. And he does think the revolution called into being a qualitatively new, and not altogether undesirable, state of affairs, as developing nations discovered an alternative strategy for planning their economies and Western elites scrambled to create social safety nets to avoid popular upheaval. Yet Hobsbawm unblinkingly describes what he calls the "unprecedented inhumanity" of Stalin's terror, as well as the increasingly poor performance of socialist command economies. He is also a great admirer of the man who, as much as any individual, helped bury the Soviet Union: Mikhail Gorbachev.

"Twenty-five years ago," observes Tony Judt, "*The Age of Extremes* would have been translated in a week." What's changed?

France, however, is a land where fashion is religiously observed, in intellectual affairs no less than in others; and intellectual fashion in France frequently comes down to single-issue politics. At least for the moment, Hobsbawm's lingering pro-Sovietism is decidedly *passé.* Shortly after *The Age of Extremes* appeared, the late historian François Furet published *The History of an Illusion,* an equally ambitious treatment of twentieth-century history—and one considerably closer to current Paris taste in its treatment of Soviet communism as "an anti-democratic and illiberal reaction" akin to fascism. Following the success of Furet's book, French publishers were apparently wary of coming out with a work like Hobsbawm's. Strangely enough, it did not occur to them to market Hobsbawm as the anti-Furet to modern history aficionados.

That's partly because French publishers are in no mood to take risks. Since the spring, there's been a great panic about what *Le Monde* loudly proclaimed *la catastrophe* in publishing. The catastrophe has some basis in economic reality: The number of books sold in the human sciences dropped from 18.2 million in 1988 to 13.2 million in 1994, and publishers have had to tighten their belts. But what's striking is how they've done it: by cutting down on translations. In an interview with *Le Monde,* Nora darkly predicted a regression to the isolationist age of his predecessor at Gallimard, Bernard Groethuysen, who didn't see a need for translations of Freud, Marx, or Heidegger. Yet Nora himself helped close the borders to intellectual exchange by refusing to publish *The Age of Extremes* in his Bibliothèque des Histoires series.

In his introduction to the symposium in *Le Débat,* Nora justified his decision by portraying himself as at the mercy of market dictates. The financial strains of publishing a book of its length (627 pages in English; 750 in French), even with help from the state, were too great and the losses almost certain. According to New Press head André Schiffren, who has edited a number of Hob-

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sbawm's books and knows Nora, it's not simply a cop-out: "Nora has lost an enormous amount of money on translations." Still, for all his fastidious bookkeeping, Nora also understood the importance of maintaining appearances. Editors, he wrote in *Le Débat*, "are obliged, for better or for worse, to take into account the intellectual and ideological conjuncture." Hobsbawm's unapologetic Marxism, he went on, "doesn't play well" with French readers.

It's not inconceivable that the French get a certain satisfaction in going it alone, even if that means absenting themselves from the international conversation. "They're putting up the drawbridges, and imagining themselves the embattled center of civilization," says Hobsbawm. "Not translating my book simply underlines the uniqueness of the French."

But the triumph of such *nombrilisme*, or navel gazing, is the French public's misfortune. Although Hobsbawm's magnanimous assessment of the Soviet experience might infuriate conservatives, his pungent critique of neoliberal nostrums about the free market would almost surely resonate with the men and women whose concerns about globalization led them to defend the welfare state and to vote the Juppé government out of office. What, then, is to be done? "Novices to dissident points of view," Serge Halimi wryly advised in *Le Monde Diplomatique*, "will have to learn English. Or one of the nineteen other languages that editorial McCarthyism has not yet contaminated."

Then again, those novices might have another option: Hobsbawm's agent is at last in negotations with a French-language publisher who wishes to bring out an edition of *The Age of Extremes*. There's one surprise, though. The willing publisher isn't French—it's Belgian.

ADAM SHATZ

CALAMITY GEORGES

EARLIER THIS FALL, A DISPUTE OVER RIGHTS TO A fifteen-acre cache of dinosaur fossils in the Montana Badlands turned nasty when paleontologists called in the F.B.I. to prevent a group of local ranchers armed with rifles and a backhoe from absconding with the bones. The ranchers, whose family had owned the land until the government foreclosed on it several years ago, hoped to cash in the bones for a small fortune—which they were almost sure to get. Three weeks later, another Badlands rancher sold his *Tyrannosaurus rex* at auction for more than $8 million.

Cuvier proved that the skeleton of the "man who witnessed the Deluge" was actually that of a giant salamander.

Though the cash incentives are new, according to a book due out next month from Chicago, the association between paleontology and violence may be at least two hundred years old. In *Georges Cuvier, Fossil Bones, and Geological Catastrophes*, a volume of elegant translations of Cuvier's scientific writings (most appearing in English for the first time), Martin J.S. Rudwick makes a novel speculation: It was no accident, he suggests, that the eighteenth-century French zoologist became an early proponent of catastrophism—the belief in sudden, overwhelming physical events on the earth's surface—in the wake of the bloody French Revolution.

A man of modest background, Cuvier's meteoric rise through the ranks of the Muséum National d'Histoire Naturelle in Paris apparently had as much to do with his having escaped the guillotine as it did with his considerable talents. Unfortunately and unfairly, writes Rudwick, a professor of history in the Science Studies Program at UC-San Diego, Cuvier is remembered today largely as an intellectual loser: a scientist who rejected evolution (in his day, the theory was called transformism) and, worse, embraced catastrophism to justify his own blinkered religiosity. In fact, Rudwick contends, Cuvier should be seen as a gifted pragmatist and skeptic who "deployed the evidence of Genesis—along with many other ancient texts—in order to bolster his *scientific* case for the reality and recent date of the physical event that, he claimed, had destroyed the entire fauna of the 'ancient world.'" Of course, we now know that Cuvier was at least half right: With the aid of something called neutron activation analysis, geologists have all but proved that a high-speed comet or asteroid slammed into Mexico's Yucatán peninsula sixty-five million years ago, causing extreme atmospheric disturbance and wiping out the dinosaurs.

Nonetheless, until he published his groundbreaking *Recherches sur les ossemens fossiles de quadrupèdes* (researches on quadruped fossil bones) in 1812, Cuvier's theory that such moldering relics were our only connection to a lost prehuman world was virtually unheard of. But the

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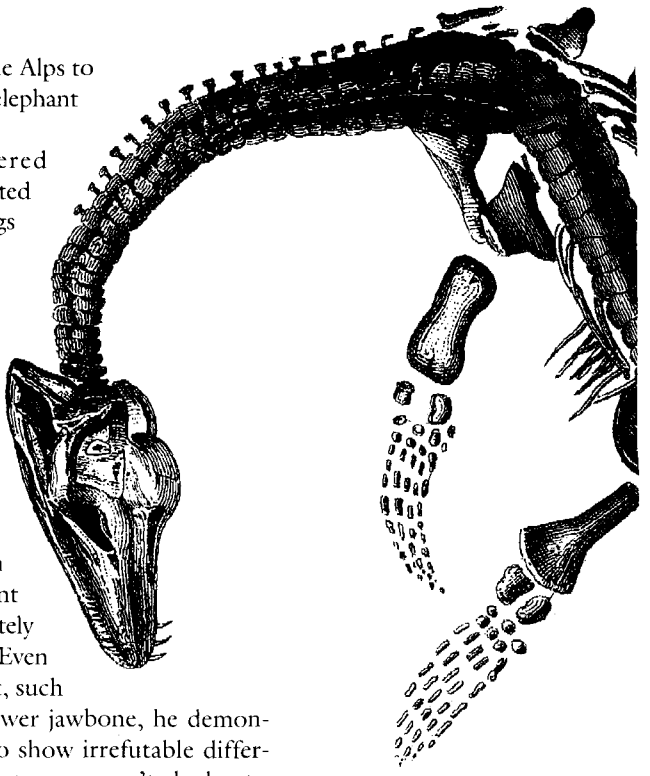
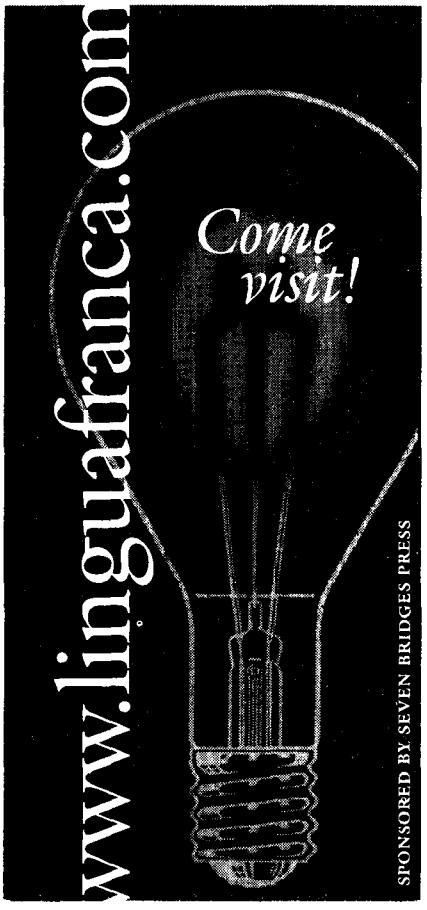
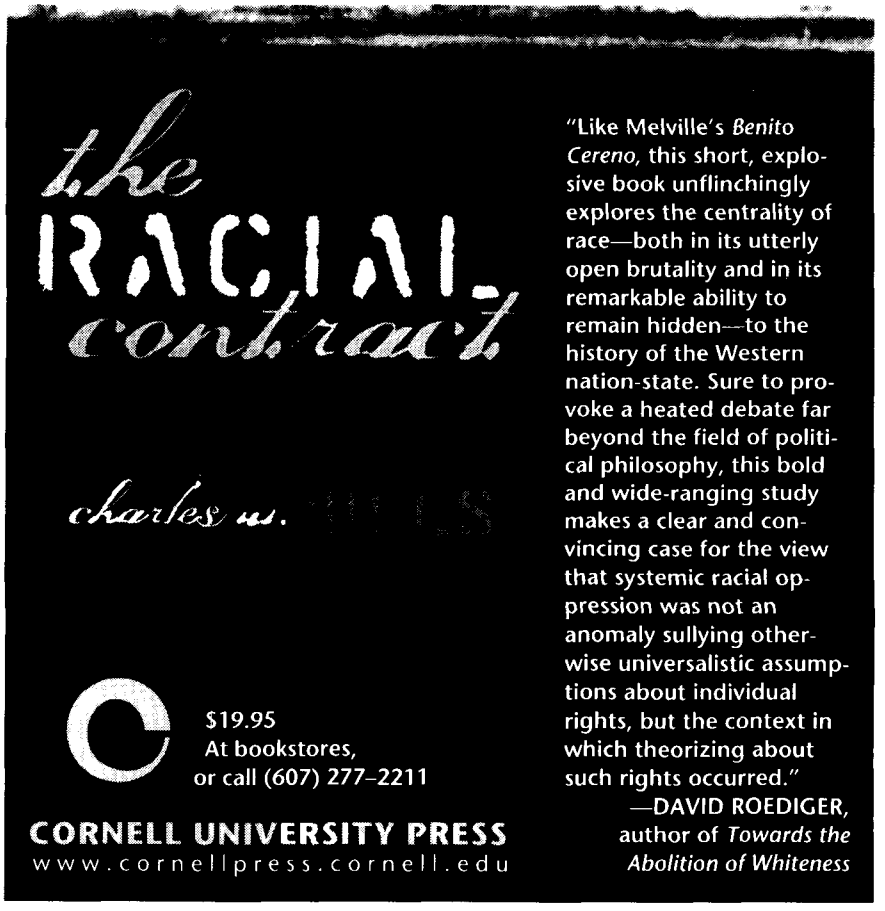
Frenchman's methodology was painstaking, tedious, and utterly persuasive. When he arrived in Paris from the provinces in 1795, during a period of unusual political stability under the science-friendly Directory government, he was an enthusiastic believer in *geognosy*, the new German earth science based on detailed empirical observation. Fossil bones, Cuvier argued, could be appraised in much the same way the Germans evaluated successive rock beds: through careful scrutiny and common sense.

In this vein, he attacked the work of armchair philosophers like the Count de Buffon, his predecessor at the museum, who believed the earth had begun as an "incandescent body" that had cooled gradually from both poles until the only remaining hot zones were at the equator. One advantage of Buffon's celebrated theory was that it explained the northern mammal fossils problem: the discovery of what appeared to be elephant and rhinoceros bones in northern Europe and North America, even though their living counterparts were confined to the tropics. Here, Buffon argued, was proof that the northern climates had formerly been much warmer and hospitable to large mammals. (Alternatively, other savants turned to classical accounts of Hannibal's legendary trek through the Alps to explain the presence of elephant bones there.)



Nonsense, countered Cuvier in a paper illustrated with anatomical drawings that he delivered to the Institut National in 1796: "A scrupulous examination of these bones, made by anatomy, will relieve us of having recourse to any of these explanations, by teaching us that they are not similar enough to those of the elephant to be regarded as absolutely from the same species." Even a single bone fragment, such as a molar tooth or lower jawbone, he demonstrated, was enough to show irrefutable differences. But if these creatures weren't elephants, what were they? By 1800, Cuvier claimed to have identified twenty-three previously unknown and now extinct species, including the mammoth,

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the megatherium (a giant sloth), and the long-headed rhinoceros.

Cuvier, of course, couldn't immediately explain what had happened to these species from the past that were no longer part of the present. Nor could he explain why species from the present were not part of the fossil record. In particular, he was troubled by the complete absence of fossilized human remains. A skeleton unearthed in 1726 and famously purported to be that of a "man who had witnessed the Deluge" (or *Homo diluvii testis*) was actually, Cuvier conclusively demonstrated, that of a giant salamander.

In a series of popular lectures at the Collège de France in 1805, and again a few years later in his widely translated four-volume magnum opus, Cuvier began to develop his theory. He daringly proposed that the present continents and life itself were new developments in the history of the earth. Moreover, he insisted, because there were no differences between living species and those preserved in the Egyptian tombs, life-forms obviously did not evolve. They simply existed in relative tranquillity until some force of inconceivable magnitude emerged to destroy them.

Always rather vague about the nature and causes of these periodic catastrophes, Cuvier was nevertheless adamant that they occurred—and suddenly. "Living organisms without number have been the victims of these catastrophes," he wrote. "Some were destroyed by deluges, others were left dry when the seabed was suddenly raised; their races are even finished forever and all they leave in the world is some debris that is hardly recognizable to the naturalist."

In hindsight, Cuvier's catastrophism seems remarkably prescient; yet he did not stumble upon the theory ex nihilo. He was not the first *savant* to make this kind of speculation, but he was certainly among the most influential, thanks at least partly, Rudwick points out, to his unusual abilities as a writer. (He is one of the few French scholars ever elected to both the Académie des Inscriptions et Belles-Lettres and its scientific counterpart.) In his conclusion, Rudwick identifies not only talent but also history in Cuvier's writing—in his repeated use of words like *détruit* (destroyed) and *anéanti* (annihilated) to describe mass species extinction. "Nature has also had its civil wars," wrote this survivor of his country's most turbulent days: "The surface of the globe has been upset by successive revolutions and various catastrophes."

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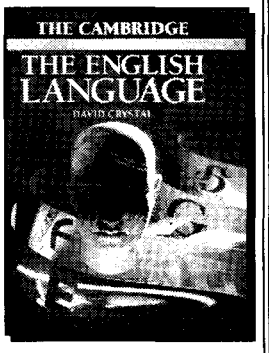

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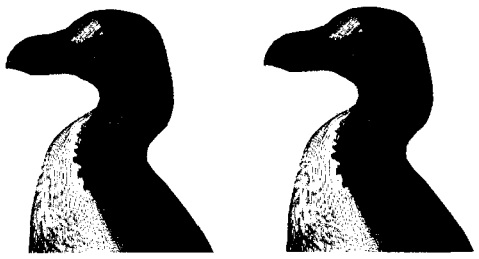

CLONING THE CLASSICS

IN JANUARY 1946, PENGUIN LAUNCHED A SMALL series of "forty Greek and Roman authors" that, it was hoped, would create a small plot of literary common ground. (E.V. Rieu's supple translation of Homer's *Odyssey* was the first offering.) But over the decades, the Penguin Classics series has become one of publishing's tallest mountains. The initial flock of forty Penguins has grown to more than nine hundred volumes—encompassing everything from Aeschylus to Zola. And that's just the black-spined breed. The seafoam Twentieth-Century Classics series has itself amassed five hundred titles in a mere decade of existence. It's enough to make you wonder: Should hunting season begin? Have too many books been given the press's Antarctic imprimatur?

Some university press editors, at any rate, wish that Penguin would rein in its boundless acquisition habits. Since the 1960s, university presses have been at the forefront of efforts to reclaim forgotten women and minority authors (as well as other worthy obscurities) for the Western canon. Such work is time-consuming and expensive: Neglected texts often need to be extensively corrected and reset. And since most of the books in series like the Brown Women Writers Project fail to attract a wide new readership, such endeavors—however worthy—typically lose big money.

That's where Penguin comes in. As some editors tell it, the British behemoth poaches their best-selling books—turning out inexpensive rivals the moment a text registers on the academic radar. And with their best titles plucked by Penguin, successful series turn into sinkholes.

Take the example of Rutgers University Press. Its American Women Writers Series is a carefully chosen list of eighteen volumes. Most remain obscure, like *The Hidden Hand* (1859), by the

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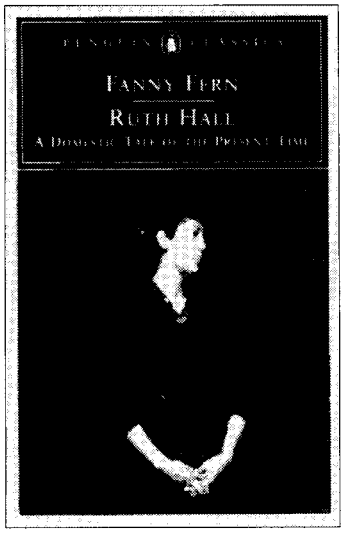

curiously named E.D.E.N. Southworth. Fortunately, two works in the series became breakout hits. The press's single-volume edition of Nella Larsen's two novellas, *Quicksand* and *Passing*, has sold more than seventy thousand copies since its 1986 publication. (Both had been essentially out of print since 1929.) *Passing* tells the riveting tale of Clare Kendry, a light-skinned black woman who has opted to renounce her African heritage and slip into white society. With its examination of the liquid boundaries of race, Larsen's novella has become required reading in many American literature seminars. "It's our best-selling title ever," boasts Rutgers director Marlie Wasserman. "It helped absorb the cost of other titles in the series, which are equally important but not as voguish."

Another surprise success was *Ruth Hall: A Domestic Tale of the Present Time*, by Fanny Fern (a.k.a. Sara Payson Willis). A fierce rebuttal to sentimental fiction of the 1850s, Fern's novel features a protagonist who ventures alone to a large American city and—instead of dying, marrying, or becoming a whore—claws her way to success as a freelance writer. The book, edited by Queens College's Joyce Warren, seems to have found a permanent home in many women's studies courses. Close to twenty thousand copies have been sold since 1986.

Alas, Rutgers's good luck was noted by editors at Penguin. This year, rival editions of both books hit stores.

What's wrong with that? Isn't the very goal of Rutgers's series to nudge obscure writers into the mainstream? Penguin editor Caroline White, who acquired *Passing*, says her mandate at Penguin is simple: "I'm trying to slowly expand the list to include more women and Latin American authors." And she's very up-front about the fact that she "sometimes turns to university press lists for fresh ideas." Even in our elastic era, it's kind of hard to call a book a classic if *nobody's* publishing it. For her part, Wasserman acknowledges that "it's totally legitimate to hire another scholar to create a rival edition. These books are in the public domain, after all. The issue is, how much effort should we be putting into plowing these fields if Penguin reaps all the benefit?"

Indeed, university press editions tend to get blown away once Penguin enters the picture. The

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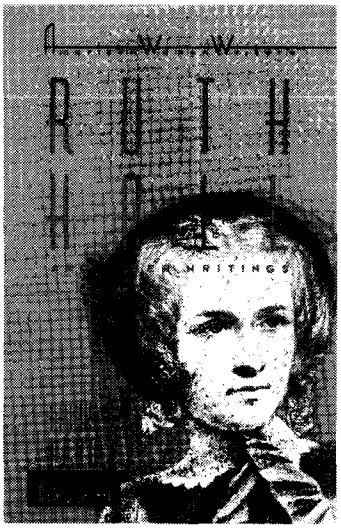
main reason: price. For years, Penguin has been engaged in an all-out price war with Oxford's World's Classics series. The competition is so cutthroat that both presses now sell many titles at cost. (In addition, bookstores willing to stock at least three hundred Penguins receive a hefty discount of 46 percent.) As a result, a typical Penguin paperback now costs around nine dollars. Rutgers's going price: seventeen dollars.

Other presses have seen successes scooped up by the Big Bird. Indiana University Press marketing manager Sue Havlish recalls her excitement upon publishing Chrétien de Troyes's thirteenth-century Arthurian cycle several years ago. "It really filled a gap for medievalists," she says. "Everyone was talking about it." But soon afterward, Penguin popped in with its own *Arthurian Romances.* End of story.

Thankfully, university press titles don't always collapse under Penguin's weight. So far, Indiana's translation of Andrey Bely's *Petersburg* has survived Penguin's 1996 challenge unscathed. "Sales are still strong," says Havlish, "because our edition is used for course adoption, and professors still like it." The danger, she says, lies with "the lay reader. Those Penguins just look so nice grouped together on your shelf."

As for Rutgers, it isn't capitulating to Penguin without a fight. "We're keeping our price artificially low, and we just ran a new ad in *PMLA,* which you wouldn't normally do for a book this old." Even so, Rutgers's sale price remains four dollars higher than Penguin's.



In the end, Wasserman merely asks that Penguin "play fair." A few years ago, she asked White to inform the press in advance of plans to duplicate books on its list. That way, she explains, "we can reduce our print runs and not end up with a warehouse full of unsold books." White agreed but never followed through. Could Wasserman really have expected Penguin to inform the competition of its plans? "All I ask is that they be considerate," she says.

It's too early to tell whether Penguin's Larsen and Fern editions will win the day. Yet, significantly, Wasserman declines to mention whether any other titles in the Women Writers Series have sales to match *Passing* and *Ruth Hall.* "I don't want to give anyone ideas," she says.

DANIEL ZALEWSKI

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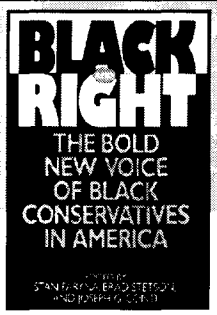

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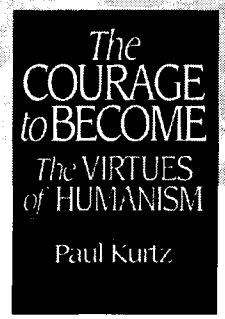

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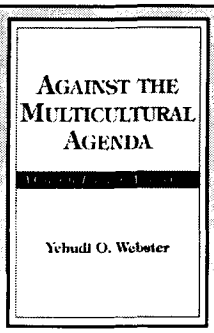

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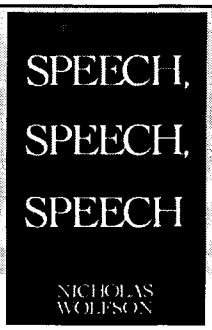

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SPIES LIKE US

WHEN SOCIOLOGISTS DECEIVE THEIR SUBJECTS

BY **CHARLOTTE ALLEN**

THEY CALL THEMSELVES GUINEAMEN. For more than two hundred years, they and their forebears have fished, hunted, raised livestock on, and otherwise made their living from a broad peninsula of marshland in a corner of the Virginia tidewater region, where the York River meets the Chesapeake Bay. Although many Guineamen now work outside the peninsula, a large number still ply the traditional Chesapeake waterman's trade, generating a distinctive local culture centered around the outboard skiff, Ford pickup, rubber wading boots, snap-brim cap, and plug of tobacco.

When I visited them one afternoon this past summer, members of two Guinea families were sitting in the yard in front of their trailer homes. When I explained to them why I was there, they began jeering and trading jibes. The target of their mockery was not another local but Carolyn Ellis, a sociologist at the University of South Florida, whose prizewinning 1986 book about the Guineamen, *Fisher Folk* (Kentucky), transformed her in their eyes from a beloved outsider and frequent guest into a traitor.

For nine years, from 1972 (when she was an undergraduate at the nearby College of William and Mary) to 1981 (when she completed her doctoral dissertation at SUNY Stony Brook), Ellis spent her weekends and summers researching a "kinship network" among a particular group of Guinea watermen. Her theory was that the Guineamen lacked the external social mechanisms—strong churches, economic cooperation, a sense of community beyond the extended family—necessary for them to prosper. The conclusions of the book were not flattering to the region, which already had a reputation for white-trash backwardness and marshland criminality.

In her writing, Ellis used pseudonyms to conceal her subjects' identities—a standard practice in sociology. Guinea became "Fishneck," and members of the local families she described were given plausible-sounding made-up names. But that didn't stop her words from causing hurt. Ellis's "Fishneckers" were often illiterate, obese, poorly dressed, and ignorant of basic hygiene. "Scarcity of plumbing meant baths were infrequent," she wrote. "That combined with everyday work with fish produced a characteristic fishy body odor, identified by outsiders as the 'Fishneck smell.'" What most riled the fishing families who had taken Ellis into their homes, fed her meals, and let her stay over on many nights, however, was that she never once let on that she was using them for sociological research. "I thought she was nice," fumes one Guinea

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woman whose family hosted Ellis often over the years. "But she turned out to be a liar."

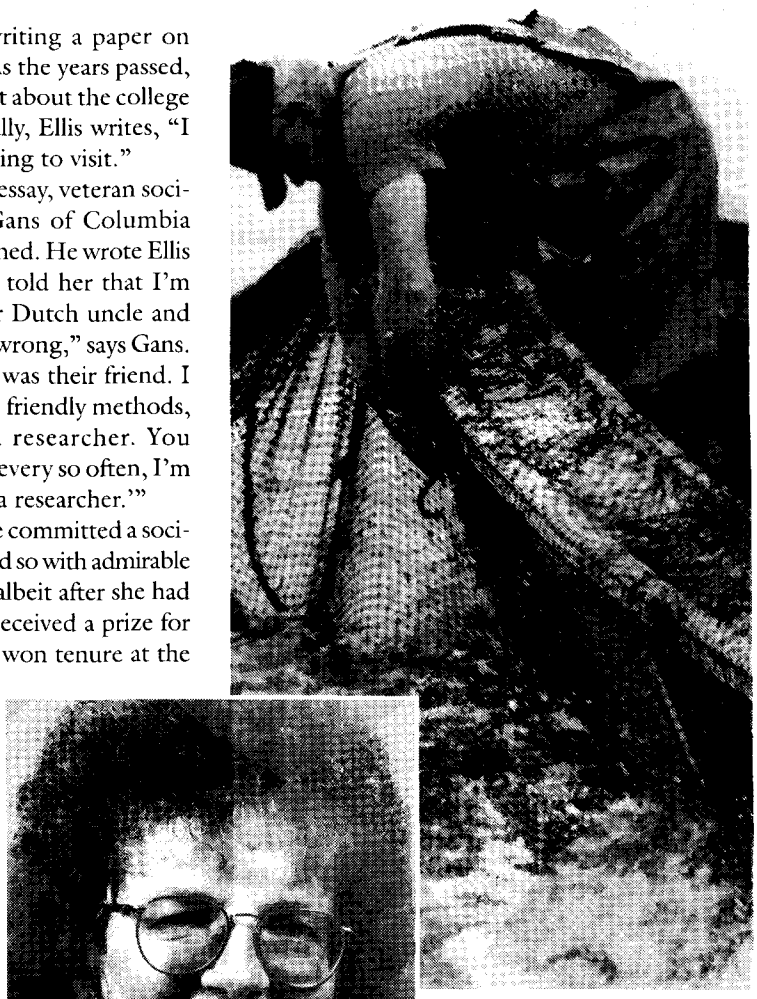
SOCIOLOGISTS have argued over the propriety of deceptive research for decades. But in 1995, the debate took a decidedly heated turn. In April of that year, Ellis published a remorseful essay in the *Journal of Contemporary Ethnography* enumerating the ways she had deceived her subjects. And her essay, which provoked much discussion among her colleagues, was not the only controversial confession that year: *The American Sociologist* published a far less remorseful account by a sociologist who some felt had used deceptive techniques to research police interrogation procedures. Finally, this spring, after two years of raging debate on the topic, the American Sociological Association (ASA) approved a set of stringent new ethics guidelines for professional conduct in the field.

More starkly than ever before, these events illustrate the degree to which the profession is caught in an uneasy bind between fulfilling research objectives and honoring ethical obligations. Sociological deception can take many forms, some more subtle than others, but all equally entangled in moral dilemmas: A researcher might not tell his subjects that he is using them for research purposes; or he might misrepresent the motives of his research; or he might violate a pledge to keep the identities of his subjects fully anonymous. In recent decades, researchers have practiced these forms of deception, and each has been earnestly defended and attacked within the profession. Ellis's behavior, it turns out, was unusual but not unique; in some ways, her deception was simply easier to see because—as she herself admitted—it was so blatant.

ACCORDING to her confession in the *Journal of Contemporary Ethnography,* Ellis secretly tape-recorded conversations with her Fishneck subjects, eavesdropped on their small talk, and coaxed data out of them while pretending to be visiting socially or doing favors, such as writing letters, baby-sitting, and driving them to doctor's appointments. "Initially, I told a number of the Fishneckers who knew I was a college student that I was writing a paper on fishing," she writes. As the years passed, nearly everyone forgot about the college connection, until finally, Ellis writes, "I was just Carolyn coming to visit."

When he read her essay, veteran sociologist Herbert J. Gans of Columbia University was concerned. He wrote Ellis a reproving letter. "I told her that I'm old enough to be her Dutch uncle and that what she did was wrong," says Gans. "She told people she was their friend. I told her, 'Yes, you use friendly methods, but you're always a researcher. You arrange to tell people every so often, I'm not your buddy. I'm a researcher.'"

Ellis agrees that she committed a sociological sin, and she said so with admirable candor in her essay—albeit after she had published her book, received a prize for it from the ASA, and won tenure at the University of South Florida. Still, she's convinced that deceiving her subjects was indispensable to her project's success. "I know I did them an injustice," she says from her Tampa office. "But I couldn't have done the study any other way. My study was predicated on my getting close to them, and if you're constantly reminding people that you're not one of them, you can't do that. They're afraid of the IRS, and I didn't want to make people suspicious of me."



CAROLYN ELLIS

Unlike Ellis, a significant number of sociologists who have engaged in deceptive research remain unrepentant. This group insists there is nothing unethical about deceiving one's subjects to a greater or lesser extent in the name of scientific research. Those who defend deceptive techniques claim subterfuge is sometimes the only way to elicit information from deviant and marginal groups—or from socially powerful groups that can otherwise justify secrecy. Defenders of deception typically use a cost-benefit analysis: If the deception doesn't hurt anyone very much and the payoff in data is high, covert research is worth doing.

Richard Leo's essay in the Spring 1995 issue of *The American Sociologist* made precisely this argument—in defiant, provocative language. Leo, then an assistant professor of sociology at the University of Colorado, boasted that he "consciously reinvented" his "persona" in order to gain admission into police interrogation rooms for research on his UC-Berkeley dissertation. Leo's larger point was that sociologists should have an evidentiary privilege—like doctors and lawyers—so they are not obliged to testify in court about what they see and hear in the field. But what struck many of his readers was his ardent defense of certain deceptive techniques. Leo declared that he had feigned conservative views (support for the death penalty, opposition to abortion and homosexuality) and had described his intimate relations with women in the same crude language he heard the cops use. In describing the ide-

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GUINEAMEN AT WORK

SHARON ZUBER

ELLIS SPORETO'S TAPE-RECORDED CONVERSATIONS WITH HER FISHNECK SUBJECTS AND EAVESDROPPED ON THEIR SMALL TALK.

ological mask he had donned in order to study the ways officers question suspects, Leo proudly compared himself to "confidence men who wish to set up their marks." Leo's article had a crusading tone: He depicted police forces as deviant groups analogous to criminal gangs who broke laws (in the case of the police it was the Miranda rule and other constitutional protections) and required extreme measures to infiltrate.

Leo's essay provoked an angry counterblast from the eminent Yale sociologist Kai Erikson, who accused him of engaging "in a degree of deceit that is more widely known in espionage than in social research." As a graduate student during the 1950s, Erikson's own ethical standards had been less rigorous. He had applied for, but failed to receive, a position on a team of undercover social investigators led by the sociologist Leon Festinger whose mission was to infiltrate a doomsday cult by lying about their professional identities and pretending to be believers. (The project resulted in a famous 1956 book by Festinger and two colleagues, *When Prophecy Fails.*) Soon after, Erikson changed his views about deceit and took an absolutist stance against it, a position he has held ever since. His arguments are both ethical and practical: It is morally wrong to lie, and it also tends to distort research. (By assuming a false persona, for instance, the sociologist forecloses opportunities to collect more complete information through direct questioning.)

Yet, as even Erikson was forced to acknowledge, Leo's case hardly constituted the most egregious example of deceptive fieldwork. After all, Leo had informed the police department (in a city that he calls "Laconia") that he was a sociologist, and he had provided the officers with an accurate written description of his project. However, he also cut his hair short, shaved off a budding beard, and put on a coat and tie before he headed for the station—which for him was decidedly out of character. He might simply have been following the dictate of Erving Goffman, who declared in his 1959 sociology classic, *The Presentation of Self in Everyday Life*, that everyone is always role-playing and there is no such thing as one's true self. "I didn't lie to them about my views," insists Leo, who now teaches at UC-Irvine. "I just didn't try to argue with them when they raised the question of abortion or homosexuality. They'd say, 'You're not against the death penalty, are you?' And I'd just laugh. I know I gave the impression that I agreed with them. I just wanted them to think I was a normal person. From their point of view, a normal person was a conservative."

Of course, it is possible that Leo, a self-identified Berkeley graduate student in sociology, fooled no one on the Laconia force with his Joe Sixpack impersonation. "These guys have fantastic bullshit detectors, if you'll pardon my French," says Robert Jackall, a sociology professor at Williams College who spent more than five years prowling crime-ravaged precincts with New York City detectives as he researched his latest book, *Wild Cowboys* (Harvard, 1997). Jackall maintains that he did not need to use deception to go where he wanted, including interrogation rooms. "I just adopted the persona given to me by the police," he says. "They dubbed me the professor. They were teaching me, and they loved the symbolic reversal. I didn't have to penetrate anything."

Leo's response is that Jackall, a middle-aged tenured professor at a well-endowed liberal arts college, had time and job security on his side, which enabled him to dispense with deception, whereas he, Leo, a penniless doctoral candidate working on a law degree at the same time, could not afford to spend more than the five hundred hours he gave to his fieldwork in Laconia. "I was a full-time student in

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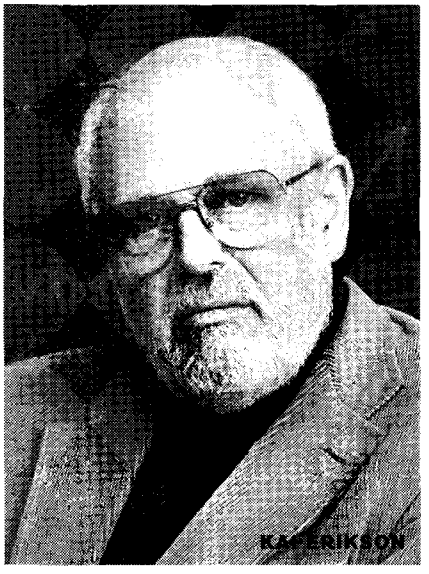
my twenties, and I just didn't have that kind of time," says Leo. "I had to get inside those interrogation rooms."

But many of Leo's colleagues aren't buying this kind of reasoning. The Leo-Erikson debate, which continued through several issues of *The American Sociologist,* resulted in a panel discussion on the morality of deceptive research when the thirteen-thousand-member ASA met for its annual convention in August 1996. Then, this past May, the ASA voted in favor of a new code of ethics that specifically addressed deceptive research techniques for the first time. The new protocol requires sociologists to obtain their subjects' informed consent "when behavior of research participants occurs in a private context where an individual can reasonably expect that no observation or reporting is taking place." Further, it explicitly bans tape-recording and videotaping without subjects' permission, as well as the use of assumed identities. Despite its hard-hitting rhetoric, however, the ethics code contains a loophole: A sociologist may obtain a waiver (from his university or the ASA) for all these constraints.

AFTER the publication of *Fisher Folk,* Carolyn Ellis was wholly unprepared for her subjects' backlash. She hoped that the Guineamen would never learn of the book's existence. Although she traveled annually to the tidewater to update her research and to visit friends among the residents, she kept mum about her monograph. "They can't read," she says. "I never took the book to them. I didn't know how to deal with it, and I hoped they would never see it."

Perhaps she underestimated the literacy rate in Fishneck. (In *Fisher Folk,* she puts it at 50 percent.) She certainly underestimated the wrath of Victor Liguori, one of her former professors at William and Mary. A specialist in maritime sociology, Liguori has spent thirty years or so working on a still-unfinished magnum opus about Guinea. He knows many residents on the peninsula, and it was he who introduced Ellis to her first Guinea contacts, as part of his custom of taking interested students with him on his research excursions. Ellis sent Liguori a copy of *Fisher Folk* upon its publication—with an acknowledgment of his help, for he had shared his research notes with her. What happened after that is a matter of some dispute.

Ellis contends that Liguori, perhaps in a fit of professional jealousy because she had poached on his academic preserve, read the most damning passages of *Fisher Folk* aloud to the Guinea unlettered, suppressing everything positive she had to say about them and generally stirring up trouble. In her 1995 article, Ellis gave



ERIKSON ACCUSED LEO OF ENGAGING "IN A DEGREE OF DECEIT THAT IS MORE WIDELY KNOWN IN ESPIONAGE THAN IN SOCIAL RESEARCH."

Liguori a pseudonym, "Professor Jack." Comparing him to a Pentecostal preacher on a Bible-thumping binge, she speculated: "Was he envious because he never finished his manuscript? Was any of his outrage justified? Or had he gone mad?" Liguori maintains that several Guineamen had obtained copies of the book, and others—who heard about it—contacted him and asked him to send them particular sections. Most of the Guineamen, he insists, read the book on their own—and then "went ballistic."

In any event, a friend eventually tipped off Ellis that several Guineamen were upset about her book, and she hastened to the marshes to beg for forgiveness. According to Ellis, after some angry exchanges about factual errors, geographical discrepancies, and broken confidences, nearly all her favorite Fishneckers forgave her. Liguori, however, contends that the Guineamen are unlikely to pardon Ellis so quickly. "One woman came up to me a month or two ago and asked, 'Is it true that she had bad things in there about the girls?'" Liguori told me in September. "And I still can't take one of my students into the marshes, especially if she's a young, attractive woman. Someone would say, 'Is she going to be another Carolyn Ellis?'" The Guineamen aren't the only ones who may be permanently shaken. Ellis herself hasn't done any fieldwork since completing her book. Her remorseful 1995 essay is representative of the work that has occupied her for the last twelve years: auto-ethnography—in which the subject is primarily the sociologist herself.

MODERN American sociology dates back to the 1920s, when the field, just beginning to get its bearings in this country, was regarded as a dubious European import. At the time, the discipline's stronghold was at the University of Chicago, where Robert Park eschewed the largely theoretical musings of his European predecessors—such as Max Weber—in favor of fieldwork based on long-term observation of one's subjects as they engage in social interaction. In Park's day, the possibility that a researcher at a local pub or political meeting might disguise his identity was virtually unthinkable.

Indeed, the ethics of deceptive research did not become a controversial topic in the profession until 1958. The occasion was a massive Cornell University study of participatory democracy in a local community and its unanticipated spin-off book, *Small Town in Mass Society,* co-written by a former project employee, Arthur J. Vidich. The project sent teams of graduate students into Candor, NY, pop. 2,500, to gather statistics. Vidich moved to Candor in order to oversee data collection and supply a friendly human face that would encourage village residents to cooperate with the survey. Now a professor emeritus at New York

MICHAEL MARSLAND/YALE UNIVERSITY

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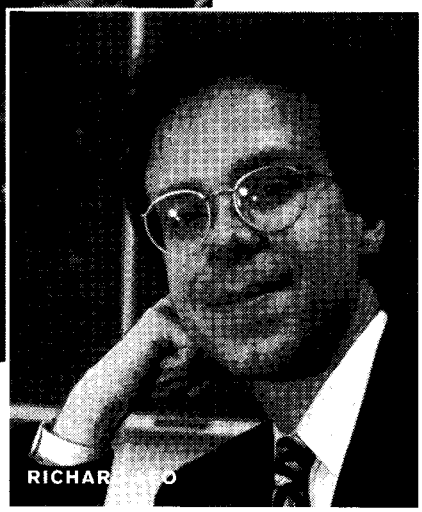
City's New School for Social Research, Vidich says that Cornell even advised him to join a local church. Although he had no interest in religion, he gamely taught Sunday school.

As part of the study, Vidich was also supposed to gather material for a more qualitative analysis of Candor's social structure. When he was hired, his supervisors showed him the code of ethics they had drafted. Vidich read it but "found nothing in it," he says today, "that related to the practical exigencies of day-to-day fieldwork. The code of ethics was a statement of intent, not a guide to conduct." (There was no provision for a participant observer like Vidich himself, for example.)



SPENCE MICHAEL WILSON (LEFT) HAZEL HANKIN/IMPACT VISUALS (RIGHT)

After living in Candor for two and a half years, he took a job in Puerto Rico, and, together with Joseph Bensman, another sociologist, used what he had learned to write *Small Town*. The book, which referred to Candor by the pseudonym "Springdale" and read like a Sinclair Lewis novel, exposed the political machinations of a clique of Springdale businessmen who ran the town behind its facade of folksy democracy. Springdale was supposed to be proudly self-reliant and scornful of urban ways, but Vidich and Bensman pointed out that the town relied heavily on federal and state intervention and was pervaded by mass culture. As they elaborated Springdale's political and social structure, Vidich and Bensman described specific townspeople and the roles they played. Although the sociologists did not use anyone's real name, it was clear to everyone in Candor who these figures were. The book became a local best-seller, à la *Peyton Place*—and a source of general outrage among residents. Vidich was hanged in effigy, and the village's Fourth of July parade featured a float carrying an image of him bending over a manure spreader.

For many years afterward, sociologists, who feared that Vidich's conduct had jeopardized the field's newfound respectability, argued over whether he had done anything wrong. On the one hand, everyone in Candor knew he was the field director of a Cornell research project. On the other hand, many Candor residents might have thought (and been encouraged by Cornell to think) that the project consisted solely of the field-workers' demographic survey.

In the end, sociologists failed to resolve the ethical questions that Vidich's course of action raised. "You have to remember that things are never quite all they seem," says Jackall, a close friend of Vidich's. "Research subjects are also trying to use the research for their own agendas and aggrandizement. People simply forgot that [Vidich] was a researcher." Vidich himself remained unrepentant. In a 1964 essay (reprinted in later editions of *Small Town*), he railed against imposing ethical restraints on social scientists. "It would be dangerous for the freedom of inquiry," he wrote, "if the formalized ethics of bureaucracy prevailed or predominated in all research."

THE deception debate shook the profession a second time in 1970, and this time the fallout left permanent damage—at least in one well-regarded sociology department. That year, Laud Humphreys, an Episcopal priest-turned-sociology graduate student at Washington University in St. Louis, published *Tearoom Trade*, a study of homosexual encounters in men's rooms (called "tearooms" in gay slang) at public parks. To gather data for his doctoral dissertation on rest-room sex, Humphreys pretended to be gay, and assumed the role of voyeur and "watch-queen"—or lookout—for the police. He also wrote down the license-plate numbers of participants in order to obtain their names and addresses. Then he waited a year, disguised his appearance, and interviewed about fifty of the tearoom regulars at their homes (sometimes in the presence of their wives and children), on the pretext of administering a social health survey. His descriptions of this second encounter made it possible that many of the men and their families would recognize themselves once the dissertation was published as a book. Humphreys cited situation ethics—the application of rules of conduct on a case-by-case basis, a popular topic at theology schools during the late 1960s—as a justification for his modus operandi. The controversy over Humphreys's covert techniques ulti-

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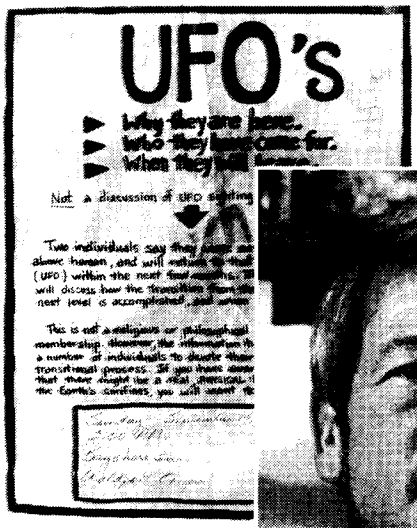
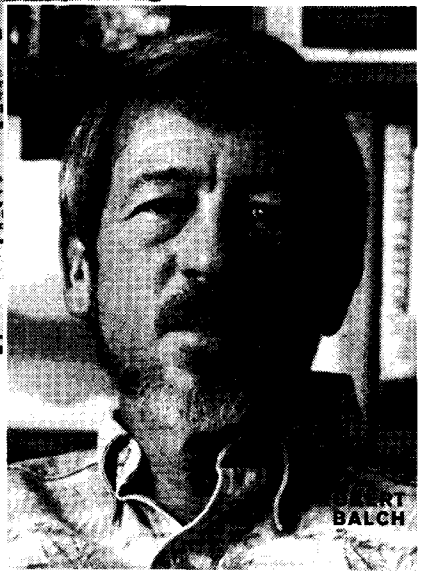
HEAVEN'S GATE LEADERS, 1975





ALBERT BALCH

mately spelled the end of sociology at Washington University. There was talk of revoking Humphreys's doctorate, and one well-known member of the department, Alvin Gouldner, delivered a blow to Humphreys's head that hospitalized him overnight. As a result, Gouldner was stripped of his title, Max Weber Research Professor of Social Theory. The sociology department never recovered from the demoralization brought on by the Humphreys incident, and, in 1989, the university disbanded the program.

Alarmed by increasing reports of unethical research practices on campuses, the Department of Health, Education, and Welfare (HEW) issued a stern report on sneaky bio-medical and behavioral research in 1978. The report came in the wake of the St. Louis scandal and adverse publicity over the filmed experiments that Yale psychologist Stanley Milgram carried out between 1960 and 1963. In his most famous work, Milgram told volunteers they were participating in a learning experiment in which they would "punish" (by means of remote-control electric shocks of ever increasing voltage) students in another room who failed to match word pairs correctly. The shocks were imaginary; Milgram was actually testing the volunteers' willingness to follow orders, which many of them did punctiliously. Milgram's film of his experiments, grainy black-and-white footage aptly titled *Obedience,* depicts its unwitting subjects as analogous to Nazi concentration-camp guards. It is shown to this day in many undergraduate classrooms.

The HEW report led to federal regulations requiring all scientists who use government funds to conduct research on human beings to clear their procedures with institutional review boards or human-subjects' committees at their universities. The boards are supposed to ensure that subjects give informed consent and to approve any exceptions to this rule. (Richard Leo, for example, got permission from UC's Human Subjects' Committee for his dissertation research on police forces.) The new ASA ethics code advises sociologists seeking waivers of its informed-consent and deceptive-research guidelines to clear their projects even when they are not using federal money.

THE CREATION of human-subjects' committees and the ASA's ethics protocol may force researchers to think twice before using deceptive techniques on a project. But neither innovation addresses the bigger questions that have dogged sociologists for years: When is deception of subjects permissible in social-science fieldwork? Should it ever be?

"We do cost-benefit analyses to justify deception," says Yale's Kai Erikson. "But most often it's we who get the benefit and they who pay the cost. There have been sociologists who have gone into religious groups or Alcoholics Anonymous. We

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ROBERT BALCH INFILTRATED A UFO CULT. "WE HAD TO TAKE NOTES IN BATHROOM STALLS, SO WE HAD TO GET UP EARLY AND WRITE THEM ON LITTLE SCRAPS OF PAPER," HE SAYS.

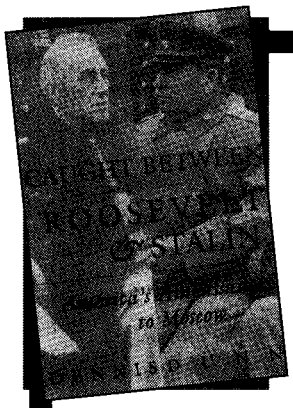
don't know how much harm it does to research subjects. There are some people who say, 'I'm doing it for the sake of science.' They're doing it for themselves. One of the things that I've noticed is that people who disguise themselves are always looking at groups less powerful than they are. If a doctor pretends to be a patient, that's all right, we say. But if a patient pretends to be a doctor, he'll get arrested."

Erikson's observation clearly applies to Ellis's relationship with the generally less educated, rural Guineamen, but not all researcher-subject relationships favor the more powerful party. Before starting to work on *Wild Cowboys*, for example, Robert Jackall published *Moral Mazes* (Oxford), a 1988 study of managerial ethics at a large (and pseudonymous) chemical-manufacturing company. Jackall ran into trouble starting his research because thirty-six corporations had flatly turned down his request to study ethics on their premises. As a desperation measure, he worked with a public-relations expert to devise a project description that would sound acceptable to a CEO. Eventually, he found his way into a chemical company that encouraged him to study the effect of chlorofluorocarbon regulation on corporate practices. Jackall took a crash course in chemistry from a fellow Williams professor, and he was soon inside the corporate doors asking questions about ethics.

His findings appeared first in a 1983 article in the *Harvard Business Review* and later in his book. Jackall concluded that the main "ethic" governing managerial practice was self-interest: protecting one's derriere and furthering one's career. He also found that organizational life was indeed a maze, a thicket of never-ending status jockeying and euphemistic doublespeak. (He included a glossary of job-performance-evaluation lingo, in which "quick thinking" meant "offers plausible excuses," and "requires work-value attitudinal readjustment" meant "lazy and hardheaded.") Jackall started receiving phone calls from managers deep within the company (and other companies) congratulating him for his acuity, but the top dogs demanded to know why he had been allowed on the premises. "All the managers had to do was pull my proposal out of the file and say, 'We thought he was here to study chlorofluorocarbon regulation,'" explains Jackall, adding that what looks like deception can sometimes be part of an elaborate linguistic code in which no one is really fooled and nearly everyone is satisfied—not least because there is always someone else to blame for the researcher's unflattering revelations.

Nonetheless, Kai Erikson maintains that deception of any kind is bad for the profession. "It jeopardizes the reputation of all the rest of us when some of us sneak around," he says. "And it's also very poor research." In her 1995 essay, for example, Ellis conceded that some of her book's ribald facts about the Fishneckers' sex lives might have been tall tales. There are other, more horrifying stories of deception gone awry: sociology graduate students who checked themselves into mental hospitals or joined cults—only to discover that the people they were observing were other sociology graduate students.

After infiltrating the UFO cult that evolved into Heaven's Gate, Robert Balch, a sociology professor at the University of Montana, came to conclusions similar to Erikson's about the morality and practicality of undercover research. Ironically, Balch's concern was not about unfairly harming his subjects but about inadvertently helping them advance an ethically dubious cause. In 1974, he became intrigued by the flying-saucer-obsessed organization, which he thought might be linked with the disappearance of twenty young people in Oregon. The following year, he and a graduate student approached members of the group as researchers with some



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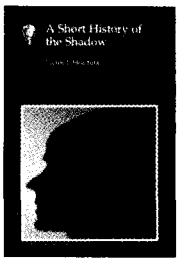

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DESPITE TROUBLING EXPERIENCES USING DECEPTIVE TECHNIQUES, FEW SOCIOLOGISTS BELIEVE COVERT RESEARCH SHOULD BE BANNED.

general questions. When the cult refused to cooperate, Balch and his student spent two months posing as members, traveling with the cult from town to town in the West as it promoted its beliefs to susceptible crowds. "We were expected to do things that we didn't want to do," Balch recalls. One evening he was obliged to promote the cult to an audience of ninety people. On another occasion, he found himself talking to a couple who had driven thousands of miles looking for the UFO group. "They had a kid," Balch recalls, "and I had to tell them, 'If you join the group, you have to leave your kid behind.' That was enough to persuade them not to join—but what if they'd decided to give up the kid?"

As undercover investigators, Balch and his student were subjected to the same unwritten rules that bound everyone else in the cult: no idle socializing (all references to one's past life were forbidden). "We had to take notes in the bathroom stalls, so we had to get up early and write them down on little scraps of paper," says Balch. "I came away with the feeling that it wasn't ethical, and it wasn't the best way to get accurate information. I wouldn't trade the experience for anything, but on every other study that I've done, I've identified myself as a sociologist."

In the end, despite troubling experiences using deceptive techniques, few sociologists believe in hard-and-fast bans on covert research. Erich Goode, a sociology professor at SUNY-Stony Brook who sat on the ASA's deceptive-research panel in 1996 with Erikson and Leo, says that the decision boils down to a trade-off: "Less-than-complete honesty versus getting the information. Do you announce up front that you're a sociologist, say, when you're studying drug dealers?" Goode believes social scientists should be free to make the trade-off at their own discretion. Accordingly, he has not sought federal funding (with its accompanying constraints) for one of his favorite covert research projects: placing bogus personal ads in order to study the sociology of mate selection. In one experiment, he placed four different ads in four different publications, two purporting to be from women seeking men and two purporting to be from men seeking women. To do this, he invented four personae: a beautiful waitress, an average-looking female lawyer, a handsome taxicab driver, and an average-looking male lawyer. One need not be a sociologist to guess the breakdown of the nearly one thousand responses, the majority from men, that Goode received (and tabulated in several scholarly articles). The beautiful waitress was the overwhelming favorite for male respondents; women preferred the average-looking male lawyer (but not by so great a margin). Originally, says Goode, "I tried to do this kind of research aboveboard. I wrote to a couple running a newsletter focusing on personal ads and explaining that I was a sociologist, but I got no reply."

GOODE'S attitude—that the knowledge gained can sometimes justify the deceitful means—may not dominate the profession today, but it represents a powerful challenge to absolutists like Kai Erikson. And it represents a faction of sociologists who are unlikely to be content with the ASA's stringent professional guidelines or with guilty, after-the-fact conversions like Carolyn Ellis's. As for Ellis, she has switched her main appointment at South Florida to communications (although she has retained

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a joint appointment in sociology). Her current projects fall under the rubrics of either auto-ethnography or "emotional sociology"—a brand-new subfield in which, as she describes it, the "emotionality of the researcher" plays a central role in the study.

In her recent essays, Ellis puts many of the emotional events of her life on display, including her abortion and her brother's death in the Air Florida crash of 1982. In 1995 she published her most ambitious piece of auto-ethnography to date, *Final Negotiations* (Temple). Nearly twice as long as *Fisher Folk*, the book is a grim, often poignant account of her tempestuous nine-year-plus relationship with Eugene Weinstein, the late chairman of the SUNY-Stony Brook sociology department. Weinstein was already dying of emphysema when Ellis met him in 1975 at a faculty party, where he passed her a toke and a kiss even though he had arrived with another woman. He had a tangled marital and romantic past (he and Ellis collaborated on an article on jealousy in open relationships). Two months after their marriage, in 1985, he died.

Ellis's book chronicles many details that might seem too tragic or intimate for other writers: LSD trips, sex with an oxygen tank in the bed, Weinstein's gradual mental decline, and his painful difficulties with elimination during his last days. Besides being fearfully ill, Weinstein was a demanding, complaining patient who could not stand to be alone. Ellis gritted her teeth and endured it—and then told it all in her book. Weinstein, she says, fully supported the project.

The sociologist who once practiced her profession by telling the secrets of people she had deceived in order to get close to them is still telling secrets. This time, however, the secrets are mostly her own or belong to those closest to her. For Ellis, auto-ethnography is a solution to the ethical quagmire surrounding deceptive research. But many sociologists are likely to find it an impractical one. Is researching oneself instead of observing others rather too high a price to pay for ethical purity?

Charlotte Allen is a contributing editor of *Lingua Franca*. Her book, *The Human Christ: The Misguided Search for the Historical Jesus*, is forthcoming from The Free Press.

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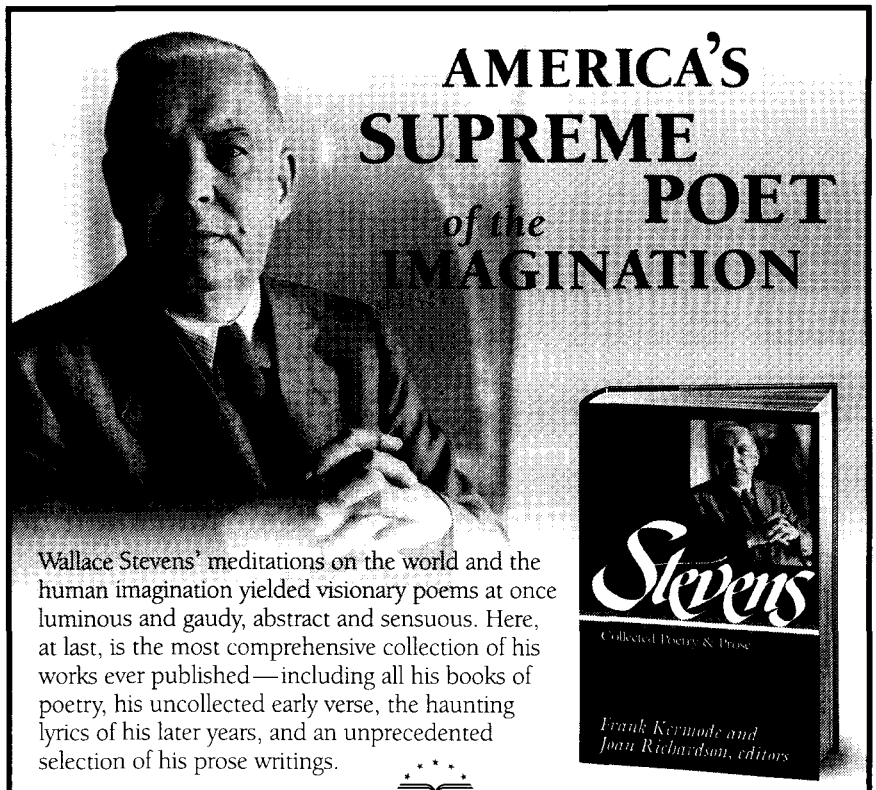

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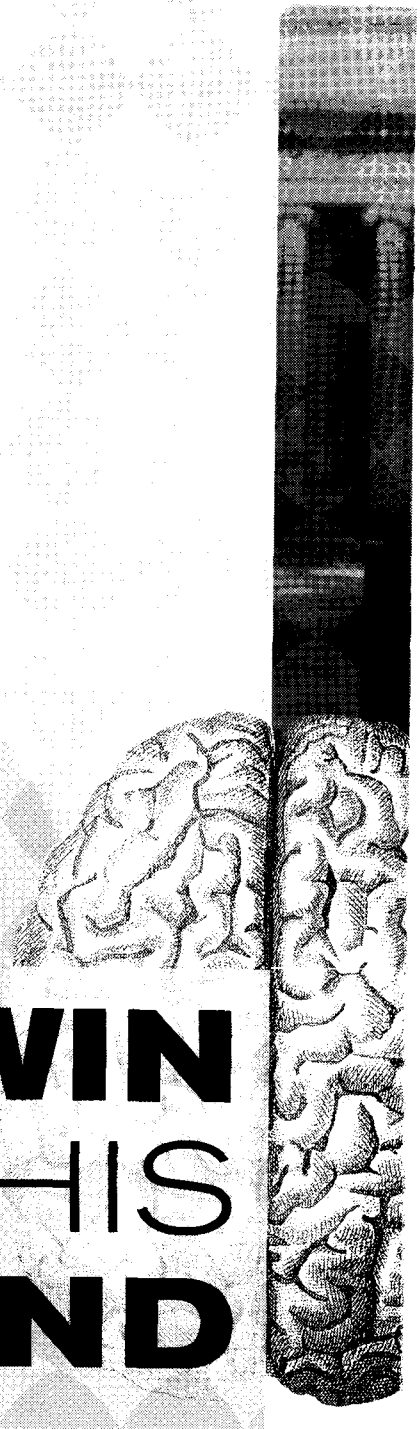

DARWIN ON HIS MIND

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EVOLUTIONARY PSYCHOLOGISTS DESPERATELY WANT RESPECT. CAN STEVEN PINKER BRING IT TO THEM?

BY **JOHN HORGAN**

AN HOUR OR SO INTO MY INTERVIEW with Steven Pinker in his airy Cambridge apartment, he offers a spanking-new evolutionary hypothesis about the latest adolescent pathology. According to a recent article in *The New York Times Magazine,* a disturbing number of teenage girls—generally from affluent families—mutilate themselves with knives, razors, fingernails. Could a hidden logic, Pinker wondered on reading the article, underlie this perverse behavior?

A girl slicing up her own forearms "is basically like a terrorist who takes a hostage that the extorted party loves," Pinker tells me, "except the hostage is herself." The girl must be unaware of her own motivation, so that she cannot be reasoned or punished into stopping. By engaging in behavior so ostensibly crazy, the girl may well force her parents to devote more time and resources to her. The payoff for all this extra nurturing? She enhances her chances of reproducing, of course. Self-mutilation, says Pinker, may in fact be a "paradoxical tactic," an activity that seems maladaptive but actually serves a Darwinian purpose.

Those who have not accepted the selfish gene into their hearts would no doubt sneer that Pinker's hypothesis is one more just-so story, an amusing but unprovable fable. After all, Pinker has merely stated what Ann Landers would have said: Some kids will do anything to get attention. But Pinker, anticipating this objection, suggests ways of testing his thesis. Does the self-destructive behavior of girls peak when they are most dependent on their parents? Does it diminish when, say, they go to college and are forced to fend for themselves? Pinker, the forty-three-year-old director of the Center for Cognitive Neuroscience at MIT, knows how to make even off-the-cuff speculation sound scientific.

Pinker's way with words has made him an academic celebrity. His 1994 book, *The Language Instinct* (Morrow), claimed that language is an adaptation, a feature that evolved in order to boost fitness and perpetuate genes. Rather than treat language as something mystical, Pinker was refreshingly practical in his argument: Proto-humans with the gift of gab simply outfoxed the strong, silent types. The ability to communicate with words was such an advantage over grunting and pointing that the grunters eventually died out. While language is an admittedly complex system, Pinker argued, it's no more complex than the eye—which virtually all scientists agree evolved through natural selection—and no more unusual than an elephant's trunk. Darwin, he concluded, explains them all.

The book became a best-seller, impressing scientists and ordinary folk alike. Pinker was hailed as the first linguist (or psycholinguist, a term he prefers because he received his doctorate in psychology) capable of writing readable prose. And in 1995, *Newsweek* designated him a member of an elite "overclass" of movers and shakers.

In his new book, *How the Mind Works* (Norton), Pinker tries to do for the entire mind what *The Language Instinct* did for language. With clever examples and buoyant prose, he floats the argument that the mind is a killer collection of "software modules," each of which performs tasks that proved indispensable on the Pleistocene savanna, from stereoscopic vision to self-deception, from face recognition to flattery. *How the Mind Works* is one of many recent books purveying Darwinian views of human nature—such as *Darwin's Dangerous Idea* by Daniel Dennett, *The Moral Animal* by Robert Wright, and *The Evolution of Desire* by David Buss—and it builds largely on the insights of others, as Pinker himself readily admits. Yet the 660-page book is a remarkable synthesis whose rhetorical shimmer alone may help transform "evolutionary psychology" (Pinker's preferred term) from a politically and scientifically suspect upstart into a dominant paradigm.

OF COURSE, *How the Mind Works* will not go unchallenged, particularly by humanists who emphasize culture's role in constructing human behavior. And not all biologists are besotted with evolutionary psychology, either. Harvard's Stephen Jay Gould, that grizzled veteran of the nature-nurture wars, recently attacked the field in *The New York Review of Books* as "Darwinian fundamentalism" and "pure guesswork in the cocktail party mode." (Pinker published an angry letter in response.) Critics like Gould believe that the work of evolutionary psychologists often suggests more about the researchers' own preoccupations than about the building blocks of human nature. Pinker may have to fend

CORBIS-BETTMANN (LEFT) BETHANY VERSOY (RIGHT)

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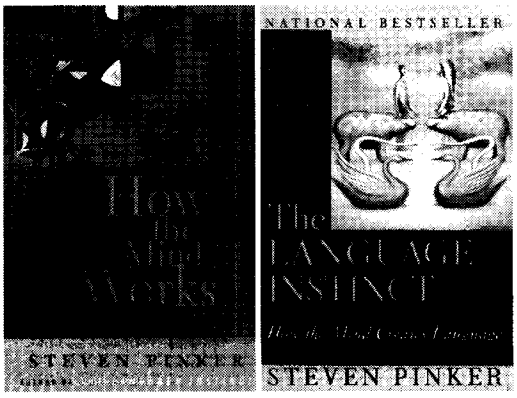
off other, less predictable foes as well. For example, his adaptationist accounts of language and other cognitive faculties set him at odds with one of his primary intellectual influences, the MIT linguist and political radical Noam Chomsky.

I have my own reasons for finding fault with Pinker. For one, he strikes me as a bit too self-consciously packaged for a serious scientist. When I interview him, he is wearing black cowboy boots, black jeans, and a shirt whose turquoise stripes suspiciously match his eyes. With his fine-boned, almost angelic face haloed by Yanni-esque locks, he looks less like a professor than an FM-lite rock star. Adding to my prejudices, in *How the Mind Works* Pinker describes a critique of evolutionary psychology I wrote two years ago for *Scientific American* as a "smear"; the article mentioned the doubts of Chomsky and others, including some evolutionary psychologists, about whether the field could ever achieve the status of a true science. But Pinker's rhetorical skills and personal charisma overwhelmed my lust for vengeance. (There must be a Darwinian explanation for that.) He is a popularizer in the best sense of the word: He makes complex ideas comprehensible, even compelling, without dumbing them down. And his writerly charm helps to neutralize the acid that is Darwinism.



LIKE *The Language Instinct,* Pinker's new book is crammed with fun facts (even a peace-loving male like the Dalai Lama loves books on warfare) and pop-culture references (a discussion of the elusiveness of happiness culminates with Jimmy Connors's remark, "I hate to lose more than I like to win"). Pinker refuses to get bogged down in neuroscience arcana; he is interested less in the mind's hardware—the neurons, neurotransmitters, and hormones that humans share with many less interesting species—than in more speculative ideas about the programs, or "instincts," that our hardware carries out. The key to his approach is the idea of "reverse-engineering." Here's how Pinker explains the concept: "In rummaging through an antique store," he writes, "we may find a contraption that is inscrutable until we figure out what it was designed to do. When we realize that it is an olive-pitter, we suddenly understand that the metal ring is designed to hold the olive, and the lever lowers an X-shaped blade through one end, pushing the pit out through the other end."

Once we accept that our minds—like other adaptations—were engineered to perpetuate our ancestors' genes, Pinker reasons, we may gain similar insights into our own thoughts, emotions, and compulsions. In *How the Mind Works,* he detects a Darwinian logic underlying everything from sibling rivalry (a competition for parental resources) to the universal fondness for slapstick (we relish seeing others lose their dignity and thus their social standing). He even makes the stale old adaptationist accounts of why men are so hotheaded and horny seem fresh. If these are just-so stories, they haven't been so stylishly told since Rudyard Kipling.

Addressing our boundless capacity for self-deception, for example, he notes that there are good evolutionary reasons for us to exaggerate our own trustworthiness, generosity, and strength. "If you are consciously aware that you are just puffing yourself up," Pinker says, "you are in danger of exposing your own lies either through autonomic ways—such as sweating, palpitating, croaking, and so on—or by talking yourself into a contradiction, which is the great problem for liars."

It is hard to imagine Pinker in such dire straits. I first saw him in the flesh in 1995 at UC-Santa Barbara, which was hosting the annual meeting of the Human Behavior and Evolution Society (HBES). Created almost a decade ago, HBES has become a haven for the swelling horde of psychologists, anthropologists, and others attempting to apply natural selection to human behavior. Many speakers sounded less like scientists than ideologues, bashing cultural determinists and others who had not embraced Darwin. Some hypotheses, such as a sociologist's claim that literature, music, and art are "primarily sexual display by young males" (he presented data on the bedroom conquests of Jimi Hendrix, among others), bordered on parody.

In contrast, Pinker, summarizing the major themes of *The Language Instinct,* was cool, authoritative, witty. (To demonstrate our hardwired ability to decode ambiguous messages, he asked us to contemplate the sentence, "Tonight, Dr. Ruth discusses sex with Dick Cavett.") That same evening, Pinker showed that he has little tolerance for imprecise reasoning—even from members of his own camp. In an after-dinner speech, Oxford's Richard Dawkins, coiner of the term "selfish gene" and an alpha male of neo-Darwinism, likened the human visual cortex to a virtual-reality computer that constructs simulations from limited information. Pinker jumped up from his front-row seat to find fault with Dawkins's analogy: A virtual-reality machine merely reduces three-dimensional information into a two-dimensional image, Pinker explained, but the visual cortex does the reverse, a vastly more difficult task. Dawkins, one of the fiercest polemicists of modern science, meekly granted the point.

PINKER DETECTS A DARWINIAN LOGIC UNDERLYING EVERYTHING FROM SIBLING RIVALRY TO THE UNIVERSAL FONDNESS FOR SLAPSTICK.

BORN in 1954, Pinker was raised in a tight-knit Jewish community in Montreal. (Although he still observes certain Jewish rituals, he no longer considers himself religious.) His mother was a high school counselor and vice prin-

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cipal, and his father was a salesman who later in life practiced law. Pinker attributes his youthful interest in psychology to the political debates of the 1960s, which "really boiled down to a view of what humans were like."

As an undergraduate at McGill University, he was steeped in behaviorism; the movement, whose best-known proponent was the Harvard psychologist B.F. Skinner, focused on animals' responses to external stimuli and dismissed internal mental states as "unobservables." ("I had a job as a rat and pigeon runner in a Skinnerian lab at McGill," Pinker recalls.) After graduating Pinker left Canada to pursue a doctorate in psychology at Harvard, where he studied visual cognition and language development.

Then Pinker discovered Chomsky. A child, Chomsky had proposed, is a "black box" that somehow transforms input from one of the world's six thousand languages into a startling ability to understand and speak that language. Chomsky argued against the blank-slate, inductive theory of learning set forth by John Locke in the seventeenth century and promulgated by behaviorists in this one. Children, he insisted, cannot learn language by merely observing and mimicking the speech of others; some innate linguistic knowledge must be embedded in their brains. Pinker was fascinated, and in 1977 cross-registered for one of Chomsky's courses at MIT. Before long, Pinker was in the linguist's thrall, and in many ways he still is. The case for Chomsky's view of language is overwhelming, Pinker says. Language is common to all cultures (unlike reading and writing), he points out, and all physiologically normal children learn to speak fluently with little or no effort, a feat that the world's fastest supercomputer cannot duplicate. Research has also shown that all languages share common structural features, such as nouns and verbs, which Chomsky has referred to as "deep grammar."

Pinker soon began putting Chomsky to the test. In one project, he visited day-care centers and pored over transcripts of conversations between parents and children, in order to study a single, simple phenomenon: the rule for creating a past-tense form of a verb. Early on, almost all children misapply the *-ed* rule to irregular verbs, saying *holded* instead of *held*, for example. Such errors indicated that children were applying—or misapplying—the kind of creative, or "generative," rule postulated by Chomsky. "*Holded* is not a form they could have memorized from their parents," Pinker tells me. Children display this creative capacity too readily, Pinker concluded, to have acquired it by postulating a rule on the basis of their experience and then applying it. "Children know things about language they could not have been taught," he tells me.

SOON after receiving his doctorate from Harvard in 1979, Pinker accepted a tenure-track position in psychology at MIT. He wrote two books setting forth neo-Chomskyan proposals about language: *Language Learnability and Language Development* (Harvard, 1984), and *Learnability and Cognition* (MIT, 1989).

And yet Pinker was not to remain merely a disciple. In fact, he would soon make a pivotal break with Chomsky. In 1988 a postdoc named Leda Cosmides applied for a job in MIT's psychology department, and she sent Pinker papers she had co-written with her husband, John Tooby, an anthropologist. In their papers, Cosmides and Tooby coined the term "evolutionary psychology" to describe their approach. Their central assertion, later published in the anthology *The Adapted Mind* (Oxford, 1992), was that just as the human body hosts a collection of distinct organs, so does the mind. Natural selection designed these innate "modules" to solve problems facing our hunter-gatherer ancestors, such as finding food and shelter, acquiring a mate, raising children, and coping with rivals. The mind is not an all-purpose machine, Cosmides and Tooby liked to say, but a Swiss Army knife, crammed with tools for different tasks.

Even the emotions formed part of this tool kit. Male sexual jealousy, the couple proposed, is a module designed to reduce the chances that a man's preferred mate will be inseminated by a rival. In her Ph.D. thesis, Cosmides presented evidence that natural selection bequeathed us a module for sniffing out potential "cheaters," those who may deceive us in some social interaction. Her research showed that people are much more adept at solving problems in logic if those problems are posed in the context of social interactions—and particularly ones that lead people to suspect they are being cheated—rather than as purely abstract puzzles. Often, people cannot say exactly why they made a correct choice; they just *know* somehow. Our sensitivity to potential cheaters is too exquisite to have been acquired through learning alone, Cosmides concluded.

NOAM CHOMSKY

Pinker recalls being "really blown away" by Cosmides and Tooby's work: "I thought it was a very impressive level of thinking and explanation." And Darwin helped fill in an enormous hole left by Chomsky: How did human beings evolve this innate talent for language, anyway?

It was a leap Chomsky pointedly refused to make. He claimed that scientists simply lacked the evidence necessary to pin down precisely why and how language and other uniquely human faculties evolved—human prehistory is too murky and the gap between Homo sapiens and other animals is too large. Understanding the mechanics of the

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modern human brain is one thing, but explaining how the brain came to be organized as it is may be impossible. (Alas, we don't have any Pleistocene brains to experiment with.) Evolutionary psychology is not a real science, Chomsky complained, but "a philosophy of mind with a little bit of science thrown in."

Others agreed. When Pinker championed Cosmides for a tenure-track position at MIT, he found few fellow enthusiasts. The appointment was promptly blocked.

BEFORE his conversion, Pinker sympathized with Chomsky's reservations about evolutionary explanations. His previous exposure to neo-Darwinism had been largely negative—coming as it did in the context of the heated 1970s debates over sociobiology. In his 1975 book, *Sociobiology,* Harvard's Edward O. Wilson had argued that the same genetic principles underlying the behavior of termites and ants might also explain human warfare, xenophobia, and the sexual division of labor. The ease with which Wilson leaped from insect societies to human ones triggered a backlash by such leftist critics as Gould and his fellow Harvard biologist, Richard Lewontin. They charged that sociobiology, like its unsavory nineteenth-century predecessor, social Darwinism, was simply another attempt to justify imperialism, sexism, and economic inequality as biological inevitabilities.

Gould and Lewontin argued, moreover, that many distinctly human faculties—both physical and mental—might be not adaptations per se but by-products of the random drift of genes or a more general increase in intelligence. For example, asserted Gould and Lewontin, language was not clearly an adaptation; it could well be the accidental upshot of our big brains.

Pinker was initially sympathetic to the Gould-Lewontin critique, in part because it dominated discourse at Harvard. Moreover, his politics during graduate school were left of center—"more so than now," Pinker says. (He coyly describes himself as "eclectic" both in his political and scientific views.) Yet he was ultimately dissatisfied with the anti-adaptationist position. "If it's all by-products, how do you get systems that see in depth, or can learn a language, or can see in color despite changing illumination, or can move arms and limbs—all of which are these horrendous engineering problems that we can't duplicate in robots?" he asks. "You couldn't get that just by random statistical drift of alleles or as a by-product of a big head!"

As for Chomsky, Pinker finds it peculiar that the man whose assertions about the biological basis of language helped spawn evolutionary psychology refuses to embrace his own stepchild. He feels that the linguist's doubts about Darwinism, despite his adamant denials, are linked to his romantic view of human nature. Rather than subscribing to the rather dark selfish-gene view promulgated by modern Darwinians, Pinker speculates, Chomsky believes that "the reason people don't always seem so creative and generous and social is that they've been corrupted by social and political structures." Chomsky's brilliance and "sheer verbal pugilism," Pinker says, have allowed him to avoid the contradictions in his position.

He flashes a devilish smile. "It's an in-joke among linguists," he adds, "that a lot of the criticisms he makes about capitalism and the mass media—namely, that the media will tolerate a narrow range of opinion and call it freedom of speech and democracy but as soon as you step outside that range you're suppressed—are not a bad characterization of Chomsky's own domination of linguistics."

PINKER made his debut as an evolutionary psychologist in 1990 in a long paper co-written with Paul Bloom, then a graduate student at MIT. Published in *Behavioral and Brain Sciences,* the widely read article forcefully rebutted the assertions of Chomsky and Gould that "the evolution of the human language faculty cannot be explained by Darwinian natural selection." Language, Pinker asserted, almost certainly *was* an adaptation; that is, it conferred benefits on our hunter-gatherer ancestors, allowing them to share learned skills related to toolmaking, hunting, and other activities. Moreover, those most adept at language would be able to manipulate others, form alliances, and enjoy advantages that would translate into more offspring. In a chatty new world, quiet simply didn't cut it.

Four years later, Pinker fleshed out those ideas and showed their compatibility with Chomskyan linguistics in *The Language Instinct,* his first book aimed at a general audience. Although Chomsky sent Pinker a critical letter about his 1990 paper, he provided a blurb for *The Language Instinct:* "An extremely valuable book, very informative, and very well written as well." Chomsky's generosity was perhaps not that surprising, considering that the book is by far the most successful popularization of his own theory of an innate language organ. (Indeed, Pinker so downplayed the role of mothers in babies' acquisition of language that he provoked *New York Times* pundit William Safire to write a Mother's Day column accusing him of badmouthing moms.)

PINKER SO DOWNPLAYED THE ROLE OF MOTHERS IN BABIES' ACQUISITION OF LANGUAGE THAT WILLIAM SAFIRE WROTE A MOTHER'S DAY COLUMN ACCUSING HIM OF BADMOUTHING MOMS.

In 1995, as a sabbatical approached, Pinker decided to undertake a vastly more ambitious project, a book that would explain virtually all of the mind's functions in evolutionary terms. His friends Cosmides and Tooby helped arrange for him to spend a year as a visiting scholar at UC-Santa Barbara, where they had become professors and co-directors of the Center for Evolutionary Psychology. In this friendly environment (MIT is still for the most part a Darwin-free zone), Pinker worked on *How the Mind Works*

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for ten hours a day, seven days a week, for a full year.

Modularity is a major theme of the book. The brain, he writes, "is not made of mental Spam" but has "a heterogeneous structure of many specialized parts." The study of brain-damaged patients revealed long ago that different parts of the brain perform different tasks, such as vision, smell, motor control, and short-term memory. Modern investigations with positron-emission tomography (PET scans) and other brain-imaging techniques indicate that large, complex modules such as language are composed of many sub-modules that carry out astonishingly specific functions. Pinker himself has helped show that our knowledge of regular and irregular verbs is stored in different parts of the brain: Certain brain-damaged patients, for example, lose the ability to remember *held* and consistently use *holded*.

The case for other modules is more inferential. For example, evolutionary psychologists speculate that our ability to infer the thoughts and emotions of others and so to predict their behavior—which is usually manifest in normal children by the age of three—may be an innate function. Autistics, who are often incapable of empathizing with other humans and treat them like inanimate objects, may have suffered damage to the "Theory-of-Mind module."

So what part of the brain harbors this key module? Nobody knows—leaving proponents of modularity vulnerable to the charge that their pet concept is more figment than fact. But modules need not always correspond to a specific region of the brain, Pinker replies in *How the Mind Works*. "Mental modules are not likely to be visible to the naked eye as circumscribed territories on the surface of the brain, like the flank steak and rump roast on the supermarket cow display," he quips. "A mental module probably looks more like roadkill, sprawling messily over the bulges and crevices of the brain."

Critics think evolutionary psychologists have taken the modularity concept too far. Steven Mithen, a British archaeologist and author of *The Prehistory of the Mind* (Thames & Hudson, 1996), complains that this emphasis reflects an overly simplistic view of human evolution. The environments in which our ancestors evolved, Mithen notes, were not monolithic but highly variable in space and time; given this fluidity, natural selection may well have favored the emergence of general-purpose learning capabilities as well as less flexible, dedicated modules. Indeed, James Fetzer, a philosopher at the University of Minnesota, has argued that science itself bears witness to the human ability to learn through trial and error.

When I bring up Fetzer's objection, Pinker displays his self-professed eclecticism. Science is obviously too modern a discipline—and too difficult for most people to learn—to be "instinctual in the same sense as language," he grants. And yet even science, he says, probably stems from highly specific modules that evolved for other purposes. The tendency of scientists to rely on visual aids supports this notion. "Why do we conceive of $y=\sin(x)$ as a squiggly line? Well, it's because we have a shape-perception module that evolved for other reasons, and we're stretching it to grasp this abstract mathematical concept in ways that make us feel comfortable."

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Occasionally, Pinker seems carried away by his own cleverness and ambition—for example, when he provides a Darwinian explanation for literature. "Fictional narratives supply us with a mental catalogue of the fatal conundrums we might face someday and the outcomes of strategies we could deploy in them," he notes in *How the Mind Works*. "What are the options if I were to suspect that my uncle killed my father, took his position, and married my mother?" What's this—*Hamlet* as survival guide? Such glib propositions seem to justify Gould's complaint that evolutionary psychology represents adaptationism run amok.

For the most part, however, Pinker is careful to present himself as a moderate in his field. Notably, he distinguishes evolutionary psychology from sociobiology. While acknowledging the contributions of the field, Pinker contends that some sociobiologists wrongly imply that the mind is adapted for maximizing fitness in all environments—including our own. In fact, he writes, the "modern mind is adapted to the Stone Age, not the computer age." As a result, our behavior is often maladaptive. For example, natural selection may well have favored primordial men who flew into a murderous rage when their mates were eyed by other men; now, men who kill sexual rivals are likely to end up in prison.

Pinker is similarly dubious about certain aspects of behavioral genetics, which attempts to find the genetic basis for mental illness, aggression, addiction, and sexual orientation, among other traits and disorders. The field is the primary source of claims—made in *The Bell Curve* and elsewhere—that differences in IQ scores between races may stem from genetic factors. Although many critics lump evolutionary psychology together with behavioral genetics, Pinker claims the two fields are radically different: The former, he says, focuses on universal features of the mind and the latter on traits that set certain individuals or groups apart. In *How the Mind Works*, he suggests that evolutionary psychology is seeking the signal—and behavioral genetics merely the noise—emerging from the cacophony of humanity: "The evidence suggests that humans everywhere on the planet see, talk, and think

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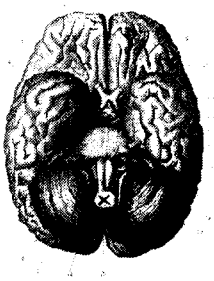

EVOLUTIONARY PSYCHOLOGISTS' "DISCOVERY" THAT MEN LIKE PRETTY WOMEN AND WOMEN LIKE RICH MEN STRIKES MANY AS BANAL.

about objects and people in the same basic way. The difference between Einstein and a high school dropout is trivial compared to the difference between the high school dropout and the best robot in existence, or between the high school dropout and a chimpanzee. That is the mystery that I want to address."

As palatable as Pinker strains to be, he can't avoid irking many feminists, for evolutionary psychology, while strongly downplaying differences among races, also emphasizes that the minds of males and females are as different as their bodies. Males, because they can in principle father a virtually infinite number of children, are much more inclined toward promiscuity than females, who can only have one child every year or so and thus select their mates much more carefully. When selecting mates, males value youth and nubility—which are markers of fertility—more than females do; females are more concerned with male status, which reflects the male's ability to provide a secure environment for children. Pinker supports this well-worn view with a quote from the owner of a dating service: "Women really look over our profile forms; guys just look at the pictures."

UC-Davis evolutionary theorist Sarah Blaffer Hrdy is one of many scientists who have questioned the degree to which such differences reflect biological rather than cultural differences; the female preference for males with money, for example, may be not instinctual but a conscious, rational response to a world in which females still suffer from a relatively low economic status. When I raise this objection, Pinker replies that empirical evidence supports the instinctual viewpoint. "What happens when you get wealthy women who don't need the money? Do they go for men with higher status than themselves or do they just go for impoverished studs? The answer is clear: Wealthy women have an even *higher* preference for wealthy men than women lower on the socioeconomic scale." Studies of successful women—including leaders of feminist organizations—have corroborated this point. Jane Fonda's coupling with media mogul Ted Turner is the rule, Pinker says, and Liz Taylor's (short-lived) partnership with bartender Larry Fortensky the exception.

Of course, feminism hasn't been around too long. Is Pinker so sure that two hundred years from now women will still be gold diggers? "I suspect they will," he says.

In any case, Pinker's feelings about feminism are double-edged. He insists that most feminists should be able to accept evolutionary psychology, especially since its findings tend to be "more invidious toward men than toward women." But as for those women who haven't embraced the *Men Are From Mars* worldview—the "true academic feminists," as he calls them—he merely throws up his hands in frustration. They

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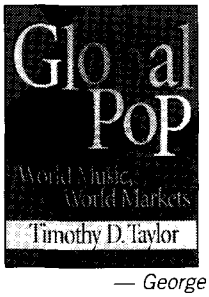

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will probably never embrace evolutionary psychology, he laments, no matter how much evidence is presented. "They also tend to be deconstructionist, postmodern Marxists," he says dismissively.

For evolutionary psychology to ever win mainstream acceptance, it must still confront another public-relations problem—the sense that it is telling us what we already know. Pinker himself has admitted that the central theme of *The Language Instinct*—language evolved because it helped our ancestors survive—probably seems rather obvious, even "boring," to laypeople. The same is doubtless true of the evolutionary psychologists' "discovery" that men like pretty women and women like rich men, or that our generosity often has a selfish component, or that children are much more likely to be abused and killed by step-parents than by natural fathers and mothers. Ho hum.

While Pinker insists that evolutionary psychology has yielded some truly surprising ideas (such as the proposal that self-deception may be adaptive), it's typically not the answers but the explanations that are novel. Take the field's explanation of kids' brattiness—an odd twist on the Oedipus complex. According to Margo Wilson and Martin Daly of Ontario's McMaster University, boys and girls should be jealous of *both* parents, since their parents' sexual relationship threatens to produce more offspring and reduce each child's share of parental resources. The children thus demand so much attention that their parents are too preoccupied (or exhausted) to contemplate sex.

AS A frequently exhausted father of two children under the age of five, I find these speculations intriguing. I am also fascinated by Pinker's remarkably bold suggestion at the end of *How the Mind Works* that evolutionary psychology, together with neuroscience and other fields, might soon achieve a kind of closure. "I see no reason that we should not have decades of progress and eventually a complete understanding" of the mind, he declares. When I ask him to elaborate, he points to two "big holes" in our current knowledge: how the brain becomes wired during fetal development and how neurons in specific parts of the brain carry out specific functions, such as perception, memory, and language. If these gaps can be filled, and if the evolutionary approach advocated by himself and others pans out, he says, "then it's conceivable that the primary work of psychology will have been done."

But Pinker, whose confidence in himself and his compatriots otherwise seems boundless, predicts that one of the deepest mysteries posed by the mind may never be solved: why neural information processing is accompanied by subjective experience, or sentience. "How could an event of neural information-processing cause the feel of a toothache or the taste of lemon or the color purple?" Pinker writes. He notes that sentience need not accompany information processing; one can certainly imagine a robot (or zombie) that precisely mimics the behavior of a human being but lacks any inner experience.

Tufts philosopher Daniel Dennett, a prominent proponent of evolutionary psychology, has derided as "Mysterians" those who think consciousness may be unexplainable. In his 1991 book *Consciousness Explained* (Little, Brown), Dennett contends that consciousness is a pseudoproblem that dissipates when viewed properly (that is, as Dennett views it). Pinker disagrees: "This is not just a superstition that gets exorcised when we have a good theory of neural information-processing."

Moreover, the Mysterian position meshes quite nicely with an evolutionary

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view of the mind. "If the mind is a system of organs designed by natural selection," Pinker asks in *How the Mind Works,* "why should we ever have expected it to comprehend all mysteries, to grasp all truths? We should be thankful that the problems of science are close enough in structure to the problems of our foraging ancestors that we have made the progress we have." Just as no rat can possibly comprehend the concept of prime numbers, he elaborates, so humans may lack the cognitive apparatus for making sense of some fundamental philosophical puzzles, including consciousness. Cosmology and theoretical physics pose similar perplexities: How can time not exist before the big bang? How can an electron be both a wave and a particle? "I think these in some sense aren't scientific questions," Pinker tells me. "Or maybe they are scientific questions that our minds are not capable of framing the answers to." Pinker attributes this notion to the philosopher Colin McGinn—who in turn has said he was inspired by the inescapable Chomsky. [See "Out of Body, Out of Mind" by Colin McGinn, *LF,* Nov./Dec. 1996.]

PINKER is under contract to write at least one more book on language, a popular treatment of his research on verbs. "This is where you go after you write a book called *How the Mind Works,*" he jokes. "You write a book on irregular participles." Pinker promises to spice up his explanations with plenty of "past-tense humor." (Example: A woman gets into a taxi at Logan Airport and says, "Can you take me somewhere where I can get scrod." The cabdriver replies, "Gee, that's the first time I've heard it in the pluperfect subjunctive.") Beyond that, Pinker might write a book about how popular but unsubstantiated assumptions about human nature shape the views of many influential intellectuals. For example, he sees shopworn Freudian concepts lurking behind the claim of a new book—*Blood Rites* (Metropolitan), by Barbara Ehrenreich—that men wage war to exorcise their primordial terror of predators. Pinker derides this model of thinking as "psychotic symbolism." There's simply no evidence, he says, to support the idea that humans have drives that must be discharged (if only symbolically) to avoid an unhealthy buildup of psychic pressure: "This is just too bizarre a way of building an organism to take seriously."

The Stanford political scientist Thomas Sowell, Pinker explains, has divided all political thinkers into two categories. On one side are those who have a tragic attitude toward human nature and believe that inequality and conflict are inevitable. On the other side are romantics, who believe that human nature is fundamentally good and therefore that all these unpleasant aspects of society may one day vanish. Evolutionary psychology, Pinker notes, obviously supports the tragic viewpoint; human nature is not compatible with completely egalitarian, socialist political schemes. "The standard Marxist theory of human nature has probably been refuted by many sources of evidence," he says, "including the anthropological record and Darwinian theory."

But when asked whether romantics such as Chomsky should abandon their dreams of eliminating warfare, poverty, and other perennial scourges, Pinker balks. He feels "very strongly," he says, that "there is a difference between *is* and *ought.* There is a realm of moral reasoning that is separate from the way the world is." Human history demonstrates our capacity for overcoming our own worst impulses, Pinker assures me. Just in the last 150 years, slavery has been abolished, women have been given the right to vote, democracy has flourished. The fact that we have come so far so fast, Pinker says, in spite of our innate limitations, suggests that there is still "much potential range for improvement." Many, if not all, of the dreams of romantics like Chomsky may yet be fulfilled.

As further evidence of how modern humans can transcend their own biological heritage, Pinker can point to his own life. In *How the Mind Works,* he reveals that he and his wife, Ilavenil Subbiah, a postdoctoral fellow in neuropsychology at Harvard, have decided not to have children, at least for now; Pinker has dedicated himself to propagating memes rather than genes. "By Darwinian standards I am a horrible mistake, a pathetic loser, not one iota less than if I were a card-carrying member of Queer Nation," Pinker writes. "But I am happy to be that way, and if my genes don't like it, they can go jump in the lake."

John Horgan is a writer living in Garrison, New York. He is the author of *The End of Science* (Broadway).

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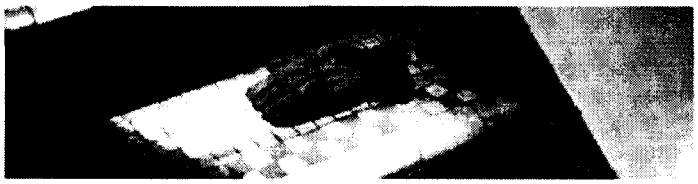

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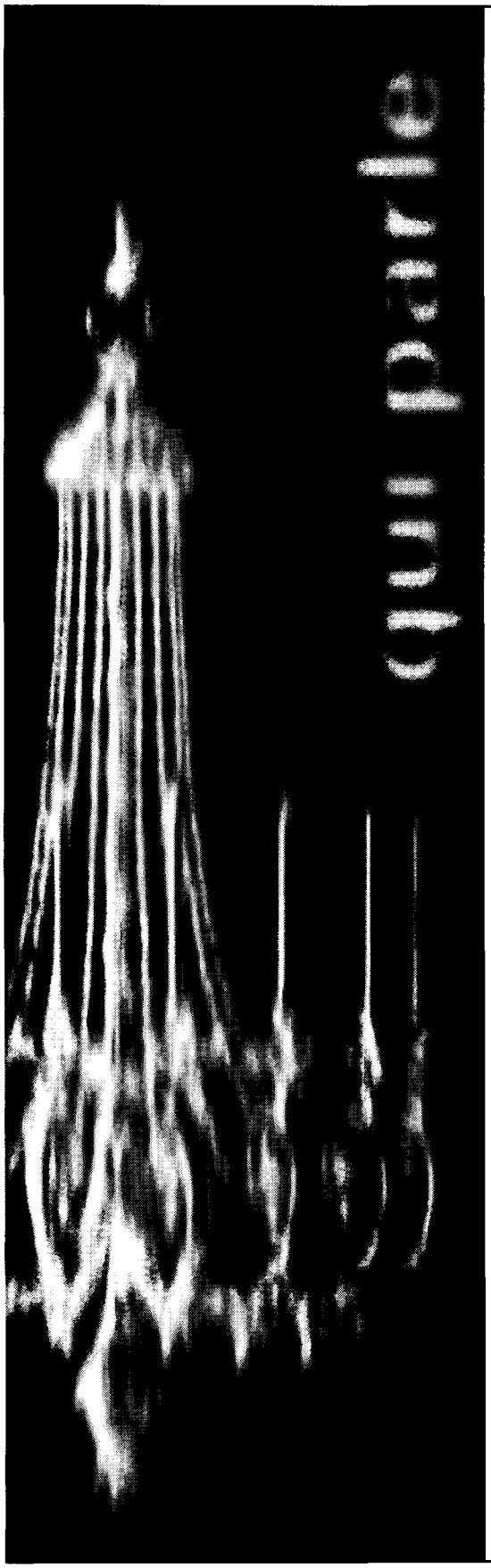

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Footnotes* seem a subject more appropriate for satire than for serious investigation. To many readers, they represent the stage on which scholarly writers perform to least advantage, displaying all the fussiness and pedantry conveyed by the term "academic" and sticking knives in as many colleagues' backs as possible. Many publishers evidently agree that footnotes repel readers. So they cut them to a minimum or banish them to the ends of books, where they may escape notice. Even many academics dislike the footnote, seeing it as little more than one of the rhetorical devices by which the scholars of older generations created a myth of intellectual authority. The rise of the academic memoir in recent years reflects some scholars' revulsion at what they see as the artificially neutral, falsely authoritative voice of the professional, footnoted text—rather like that of the commentator in a newsreel of the 1930s.

In history, however, footnotes play a vital, double role. On the one hand, they support historians' narratives, showing that their authors have done the necessary amount and kinds of research. On the other hand, they democratize historical debate, enabling and even encouraging readers to argue with authors about their interpretations of the record. They show not only that any given piece of historical writing rests on solid work but also that it is inevitably provisional; they invite criticism and revision. Without them, the argument without end, characteristic of modern, professional history, could not go on.

*BIRTH OF THE FOOTNOTE

BY **ANTHONY GRAFTON**

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Traditional histories did not identify their sources in detail, since they won credibility in other ways—from their authors' high position or demonstrated prudence, for example. They were designed to be instructive and eloquent, not learned: to teach political and moral lessons about the meaning of the past rather than technical lessons about how to reconstruct it. Modern history, by contrast, normally has two different levels of narrative architecture. Upstairs, historians tell stories and offer interpretations about the past; downstairs, they describe the course of their own research.

For the last century and more, most historians have assumed that this modern form of footnoted history came into being in the German universities of the nineteenth century, where Leopold von Ranke and others created new standards for the use of documents and new forms of professional training, like the seminar, that taught and enforced them. But this story is too simple. The characteristically modern forms of historical documentation took shape many years earlier and emerged as a partial remedy for some of the most dramatic and tormenting problems faced by the intellectuals of early-modern and modern Europe—much like the characteristically modern forms of scientific research and argument, which took shape in the same period. If the geometrical method of Descartes provided an apparently rigorous form for arguments about nature, the footnoted history written by Pierre Bayle and others provided an apparently rigorous—and yet still eloquent—form for critical discussion of the human past. The essay that follows is adapted from my forthcoming book, *The Footnote: A Curious History* (Harvard). It describes the moment when the footnote crystallized into its definitive form, in the last decades of the seventeenth century and the first decades of the eighteenth.

ONE OF THE grandest and most influential works of late seventeenth-century historiography not only has footnotes but largely *consists* of footnotes, and even footnotes to footnotes. The vast pages of that unlikely best-seller, Pierre Bayle's *Dictionnaire historique et critique,* offer the reader only a thin and fragile crust of text on which to cross the deep, dark swamp of commentary.

Bayle was a characteristic as well as a dominant figure of the French Calvinist emigration of the late seventeenth century—the wave of Huguenots, which included thousands of artisans as well as dozens of leading intellectuals, who were driven from France by religious persecution under Louis XIV. A student of the new philosophy of Descartes and an amateur, but expert, connoisseur of Protestant theology and exegesis, Bayle taught at the Protestant Academy of Sedan in northeastern France and, after it closed, at the Gymnasium Illustre in Rotterdam. But above all, he made his way as an editor and writer. His monthly journal of extended reviews, the *Nouvelles de la République des Lettres,* reached a wide public soon after it started publication, in 1684. Bayle soon found himself in possession of a well-known name and a European network of correspondents. At the same time, however, he found himself more and more in difficulties. The French authorities, who detested the ironic brilliance of this Protestant critic whom they could not reach, arrested his brother, who refused to convert. (The severity of his imprisonment proved fatal.) Meanwhile Bayle's political tolerance and personal loyalties brought him into conflict with his former friend, the Calvinist theologian Pierre Jurieu. Bayle lost his teaching post and came under sharp literary assault.

Despite these pressures, Bayle maintained his personal and intellectual independence and went on fighting smug orthodoxies on all sides (he described himself, wonderfully, as a real protestant—the sort who on principle protests against everything). But he saw that he would have to make his way by writing. Amazingly enough, a vast, unruly reference work that took him years to complete also earned him a living. Bayle set out, early in the 1690s, to create a dictionary of all the mistakes in all other works of reference, especially those in the vastly popular *Grand dictionnaire historique* of Louis Moreri (Paris, 1674), which would reach its twentieth edition, despite Bayle's criticisms, in 1759. In his *projet,* a sketch that Bayle circulated to test market and reader response, he described the task with characteristic modesty: "It's worse than setting out to fight monsters. It's trying to wipe out the Hydra's heads: at the least it's trying to clean the Augean Stables." His basic idea was as simple as it was ambitious. In collecting material about the Roman philosopher Seneca, for example, Bayle would list all the omissions and errors in existing reference books. Anything the reader learned elsewhere and did not find contradicted in Bayle would be true.

Bayle was anything but naive. He knew that controversies raged about many facts and that the reader could not always determine where truth lay. Even the harshest and apparently most credible critics committed dozens of errors. The greatest scholars of the previous two centuries—even Joseph Scaliger and Claude Saumaise—had not only discovered others' mistakes but made their own. In the course of the bitter controversies that continually broke out between historians and philologists, the truth bounced and flew as hard, and at times as wildly, as a tennis ball at Wimbledon. Only a comprehensive dictionary of errors, Bayle held, could give readers an Ariadne's thread to lead them through the labyrinthine scholarly polemics of the last two hundred years. Hurling all his metaphors, traditional and modern, into one basket, he suggested that one might call his projected book "the touchstone for all other books" and "the insurance exchange of the Republic of Letters."

The public response to Bayle's proposal took two forms: criticism from readers he respected, like Leibniz, and a vast, collective yawn from the rest. Even the immensely erudite Gilles Ménage, for example, found the proposal for a dictionary of errors unappealing, much though he respected Bayle's talent and hoped he would succeed. Accordingly, Bayle set out to produce something even grander: a historical dictionary of persons (and a few places) ancient, medieval, and modern, all of it supported by a vast apparatus of references and citations.

1 IN THE EARLY 1690s, PIERRE BAYLE SET OUT TO CREATE A

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The dictionary appeared in December 1696, was enlarged in 1702, and formed the favorite reading matter of just about every literate European for much of the next century. Students queued up to use it in the Bibliothèque Mazarine; all serious collectors bought it. Voltaire devoted an immense amount of time to reading, annotating, and reacting to Bayle's articles, to which he owed endless stimulation and productive irritation. Those who tried to combine erudition and philosophy found the book especially fascinating. The pioneering historian of art J.J. Winckelmann, another of the eighteenth-century writers who transformed the tradition of antiquarian scholarship into something rich and strange, read the dictionary twice and copied from it what he called a "full-scale book," thirteen hundred pages' worth of articles, written out in a minute hand.

It may seem odd to identify Bayle, a thinker usually regarded as the one who taught the intellectuals of the Enlightenment to doubt everything, as a founder of historical learning. Many readers have found the dictionary a vast subversive engine designed to undermine the Bible, Protestant orthodoxy, and the very notion of exact knowledge. Bayle repeatedly exposed errors and contradictions: between the despised Moreri, his predecessor in the dictionary-making game, and the sources; between the sources themselves; between the sources and common sense. He insisted that massive falsification had interfered with the historical record. All writers, pagan and Christian alike, distorted in order to condemn: "This method has been used in all times and places. Men have always tried, and still try, to ridicule the doctrine and the person of their adversaries: To achieve this they invent thousands of stories." In the dour footnote D to his account of Giacomo Bonfadius—a historian whose enemies arranged his condemnation and execution for sodomy—Bayle ridiculed the Ciceronian notion that historians should and could tell the whole truth:

PIERRE BAYLE

> Nothing is finer in theory than the ideas of the lawgiver of historians. He commands them not to dare to say anything that is false, and to dare to say everything that is true. But these are impractical laws, like those of the Decalogue, given the condition in which the human race finds itself.... In addition, let us observe a great difference between such similar laws. Only a perfect wisdom can live according to the Decalogue; and it would be a complete folly to carry out the laws of history. Eternal life is the fruit of obedience to the Decalogue; but temporal death is the almost inevitable consequence of obedience to the lawgiver of historians.

Many readers, accordingly, have seen Bayle as the sworn enemy of the notion that history could ever recover solid facts—and have interpreted the swarming irreverences of his footnotes as a massive effort to subvert all certainties.

Yet Bayle's readers could—and can—learn many lessons from him, some of which apparently contradict others. Bayle emphasized the rules of good scholarship as well as the defects of bad. In doing so he stated, formally, rules of scholarly procedure—the very rules that Gibbon and his critics took for granted late in the eighteenth century, when they bitterly debated whether the authorities that were marshaled at the foot of every page of the *Decline and Fall of the Roman Empire* supported the arguments in the text. In his entry on King David, for example, Bayle writes:

> The life of this great prince, published by the Abbé de Choisi, is a very good book, and would have been much better if someone had taken the trouble to set down in the margin the years of each action and the passages from the Bible or Josephus that furnished him his data. A reader is not pleased to be left ignorant about whether what he reads comes from a sacred source or a profane one.

Citation, evidently, must be full and precise. So must the collection of testimonies. Bayle's footnotes buzz with the salacious twaddle of the Republic of Letters, with every pornographic interpretation of a biblical passage and every sexual anecdote about a philosopher or a scholar. We owe to him the preservation of Caspar Scioppius's description of the sparrow he watched having intercourse twenty times and then dying—as well as Scioppius's reflection, "O unfair lot. Is this to be granted to sparrows and denied to men?" Readers have often wondered if Bayle hoped to hide the most scandalous and irreverent bits of his work from censorship by placing them in his footnotes rather than in the text. But it seems certain, as Walter Rex argued a generation ago, that Bayle did not try to evade detection. His most hostile readers, after all, were also habitués of works of erudition, expert explorers of scholarly apparatuses. No nook or cranny in a suspect commentary could escape their attention.

When wicked passages in Bayle's notes attracted flak from orthodox batteries, Catholic and Calvinist, he not only refused to take evasive action but also deployed a powerful defense:

> This is a historical dictionary, with commentary. "Laïs" ought to have its place in it as well as "Lucretia".... It is necessary to give in it not only a recital of the best known events, but also an exact account of the least known ones, and a collection of what has been dispersed in various places. It is necessary to bring to bear proofs, to examine them, confirm them, and clarify them. In a word, this is a work of compilation.

ICTIONARY OF ALL THE MISTAKES IN OTHER WORKS OF REFERENCE.

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The claim to be a compiler, however, amounted to more than a defense of the naughty bits of the footnotes. Bayle made compilation into a term of pride. More elegant writers who refused to provide the evidence in full had brought scholarship into discredit. Bayle's vast accumulation of passages from other texts, of exegesis, summary, and rebuttal, was a profound exercise in truth seeking—the only one, indeed, that could allay the fears of readers rightly discouraged by the usual methods of uncritical scholarship. The typical historians distorted; but the compiler, who necessarily preserved even what was distasteful, offered the critical reader as much truth as human effort could attain. Bayle described such obsessive researchers with eloquence, even fire: "They try to verify everything, they always go to the source, they examine the author's intent, they do not stop at the passage they need, but examine closely what precedes and what follows it. They try to make suitable applications, and to link their authorities well. They compare them with one another—or, indeed, they show that they conflict. Moreover, they are people who make it their religion, when points of fact are concerned, to make no assertion that has no proof." Bayle, in short, filled his dictionary not only with random, entertaining facts but also with crisp, explicit, persuasive statements of the previously developed forms of antiquarian practice. At the touch of his philosopher's stone, the lead of practice was transmuted into the gold of precepts.

Bayle clearly thought that the redoubled, footnoted form of his work made it radically new; he believed that he had departed from the literary rules of the game. He explained that he had had to sustain "in this mass of all sorts of things a dual personality, that of historian and that of commentator." As a historian, he recounted in the text his countless odd, ill-chosen stories of the lives and deaths, the views and bizarreries of thousands of individuals. As a commentator, he told his readers, he had tried to "compare the arguments for and against something, with all the impartiality of a faithful reporter." Bayle devised and defended a double form of narrative, one that both stated final results and explained the journey necessary to reach them. Pressed by a thousand enemies, enraged at the reign of error in a thousand books, and unsupported by any institution, Bayle had only the authority of his own scholarly workmanship to rely on. The format he chose reinforced his criticisms of error as nothing else could have—and gave him, as it would Gibbon, endless space for subversive ironies.

ALL authors who addressed controversial questions in the years around 1700 knew they were entering minefields: Footnotes naturally appealed to many of those who discussed historical and philological topics as the best way to protect themselves against hidden and overt attack. But other social and cultural conditions also helped to make intellectuals self-conscious about the problem of authority in writing about the past—and, in Bayle's case, sharply articulate about the way to avoid disaster. The seventeenth century, after all, saw the scientific authority of the ancients deconstructed by Bacon, Descartes, Boyle, and Pascal; the political authority of kings deconstructed by French Frondeurs and English Puritans; and the historical authority of the Bible deconstructed by La Peyrère and Spinoza. Questions of authority and evidence posed themselves on every side. Whose descriptions of the behavior of a barometer or a comet, a new substance or a new island, deserved belief? What made one account authoritative and another implausible? Any intellectual of the late seventeenth century necessarily confronted these and other questions of intellectual authority—and had to devise protocols for providing assurances that could quell the doubts of skeptical readers.

Students of the past, however, faced special problems. Bayle, as Carlo Borghero has shown, was one of dozens of European scholars who were forced in the later seventeenth century to resist not only the normal forms of clerical intolerance but also a far more fundamental attack on their whole discipline. Descartes's vastly influential *Discours de la méthode,* of 1637, included a withering critique of historical knowledge as well as a program for a new philosophy. Descartes dismissed history and the humanities as pastimes no more informative or rigorous than travel (both showed only that human opinions and customs diverged endlessly). But he also supplied his opponents with weapons that could be used against him. In both his mathematical and his philosophical works, Descartes made clear that the formal qualities of mathematical arguments lent them the rigor and generality that humanistic ones lacked. Some defenders of historical knowledge, like Pierre-Daniel Huet and John Craig, applied this argument directly to their work. They tried to make their historical criticism proof against skeptical attack by casting it in the Cartesian or Newtonian form of quasi-geometrical chains of deduction. Craig, for example, went so far as to devise formulas for measuring the decrease of authority over time that the testimony of any witness must undergo. He even computed the date at which the witnesses to the life of Christ lost their credibility.

Bayle and his fellow footnoters responded to Descartes in a more constructive way. They not only applied but stated the rules that verified or falsified historical propositions. And they created the form of the double narrative, as one that would make explicit, just as the Cartesian *Regulae* did, that each argument followed from all the relevant evidence. Indeed, to become modern, philology needed the unkind assistance of philosophy. Bayle needed Descartes.

The early history of Bayle's project for his dictionary supports this analysis. In his *Projet,* Bayle insisted that his work would find many readers, precisely because the sciences of historical and philological criticism were flourishing as never before:

> I do not wish to be told that our century, which has been restored and cured from the critical spirit that reigned in the preceding one, regards as mere pedantry the writings of those who correct errors

2 BAYLE'S PRACTICE DID NOT ALWAYS LIVE UP TO HIS PRINCIPLES CITED BOOKS FROM MEMORY, AND OFTEN GAVE INCOMPLET

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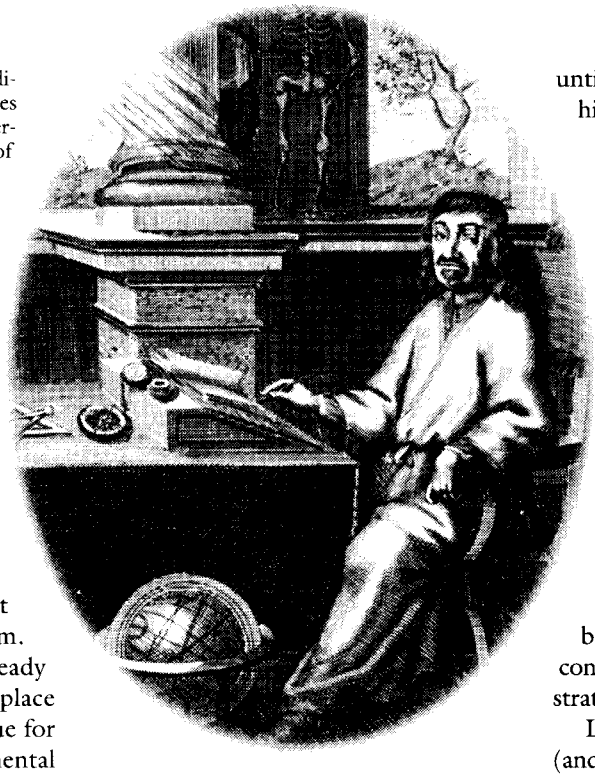
> of fact, with regard either to the individual lives of great men or the names of cities or similar points. For it is certain, on the whole, that this sort of enlightenment has never found more support than it does at present. For every investigator of physical experiments and every mathematician you will find a hundred profound students of history and all its subdisciplines. The science of antiquities, by which I mean the study of medals, inscriptions, bas-reliefs, and so on, has never been cultivated as it is now.

Bayle's voice sounds proud and positive here. But he introduced this passage, revealingly, as a response to a hypothetical objection. He knew perfectly well that popular opinion was against him. Many dedicated scholars had already despaired of regaining a central place in the curriculum, given the vogue for Cartesian philosophy and experimental science. That, in turn, explains why Bayle felt it necessary to argue at length against the fashionable view that mathematics had an advantage over historical knowledge in that it "leads us to truths not susceptible of doubt." On the contrary, Bayle insisted, the "certitudes" of history, though different from those of mathematics, were far more concrete, more applicable to human life, and even "more certain, in a metaphysical sense," than "the profound abstractions of mathematics."

In this same sketch, but in another context, Bayle acknowledged that problems of citation played an important role in making history seem less certain than it was:

> If an author asserts things without citing his source, the reader has occasion to believe that he speaks only on the basis of hearsay. If he does cite, the reader fears that he quotes the passage wrongly or misunderstands it.... What, then, is to be done in order to remove these reasons for mistrust? There are a great many books that have never been refuted, and a great many readers who do not possess the books that contain the full series of literary disputations.

The last of the draft dictionary entries that Bayle printed with his *Projet* was the hilarious "Zeuxis," with its brilliantly ironic account of the difficulties that had confronted the great Greek artist when he asked to see his models naked. In footnote A, Bayle insisted on the positive importance of proper citation. As usual, Moreri had gone about the matter in exactly the wrong way: "He piles up all his citations at the end of each article, without informing us that a particular author said one thing, and a second author another. He thus gives his reader a great deal of trouble: One must sometimes knock at five or six doors before finding someone with whom one may speak." The same point, Bayle noted with pleasure, had already been made by the ecclesiastical historian le Nain de Tillemont, a favorite source of Gibbon's, whose own works consisted for the most part of extracts from sources. Bayle praised Tillemont's "method of citation" as "of the utmost exactness."



RENÉ DESCARTES

Evidently, Bayle believed that his dictionary was connected with the defense of the historical sciences and that the proper mode of citation was vital to that enterprise. But the full connection apparently did not become clear even to him until the most erudite and brilliant of his critics tied these threads of argument and practice together. Leibniz, in his response to Bayle's *Projet*, discouraged his erudite correspondent from compiling a list of errors or a doxography of scholarly debates. But he agreed with Bayle "that those pure mathematicians and physicists, who are ignorant of and despise all other forms of knowledge, are wrong." And he insisted that a trimmed-down and reoriented version of the project, one that addressed itself to truths rather than errors, would be very useful. Vital to this reference work would be a form of citation designed not to confuse the reader further but to demonstrate conclusively where the truth lay.

Leibniz was an experienced editor (and entrepreneur for editions that lesser men carried out). He gave Bayle crisp, specific advice:

> I suppose that the best way to attain this end would be to speak about the subject in question, and generally to quote the passages from texts, on which one relies, often giving the authors' exact words, in imitation of M. Du Cange's excellent work. It will be possible to put these words in the margin, since generally there seems to be some reluctance about putting Greek or Latin directly into the French text. If you had set out to produce a work in Latin, you would have more freedom in this respect, for in points of fact there is nothing like seeing the authors' own words.

The close connections between philosophy and philology emerge clearly here, as Leibniz and Bayle cast about for models of accurate citation in the philological literature of their time. So does the high quality of Catholic learning, as evidenced both by Bayle's reference to the Jansenist church historian Tillemont and by Leibniz's to the huge dictionaries of Byzantine Greek and medieval Latin by Charles Du Cange.

Most importantly, Bayle arrived at his new method of citation after engaging in sustained reflection and debate. Footnotes mattered to him—mattered

LIKE HIS ENEMIES, HE SILENTLY ABRIDGED QUOTATIONS, BIBLIOGRAPHICAL DETAILS IN HIS OWN FOOTNOTES.

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The Passion of David Lynch

Wild at Heart in Hollywood

By Martha P. Nochimson

Filmmaker David Lynch asserts that when he is directing, ninety percent of the time he doesn't know what he is doing. To understand Lynch's films, Nochimson believes, requires a similar method of being open to the subconscious, of resisting the logical reductive-ness of language. In this innovative book, she draws on these strategies to offer close readings of Lynch's films and television productions including *Twin Peaks: Fire Walk with Me, Wild at Heart, Twin Peaks, Blue Velvet, Dune, The Elephant Man, Eraserhead, The Grandmother, The Alphabet,* and Lynch's most recent, *Lost Highway.* Nochimson's study is informed by unprecedented, in-depth interviews with Lynch himself.

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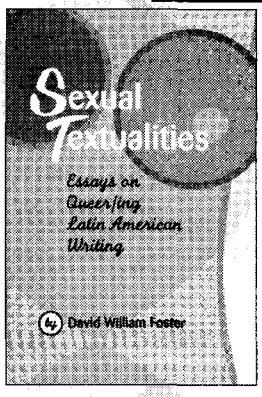

Sexual Textualities

Essays on Queer/ing Latin American Writing

By David William Foster

Moving beyond Foster's groundbreaking book *Gay and Lesbian Themes in Latin American Writing, Sexual Textualities* analyzes questions of gender representation in Latin American cultural productions to establish the interrelationships, tensions, and irresolvable conflicts between heterosexism and homoeroticism. The topics addressed include Eva Perón as a cultural/sexual icon, feminine pornography, Luis Humberto Hermosillo's classic gay film *Doña Herlinda y su hijo,* homoerotic writing and Chicano authors, Matías Montes Huidobro's *Exilio* and the representation of gay identity, representation of the body in Alejandra Pizarnik's poetry, and the crisis of masculinity in Argentine fiction from 1940 to 1960.

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enough not only to be compiled with endless energy and laced with sardonic humor but also to be the object of serious epistemological effort. Whatever his ultimate intentions, then, Bayle shored up the very historical discipline that many have seen him as challenging. True, his practice did not live up to his principles. Bayle, like his enemies, silently abridged and consciously or unconsciously misread the texts he instructed his printers to excerpt (he tried to avoid copying out long extracts, which he saw as a waste of time, even though he thus apparently violated his own strict principles for the critical use of sources). Though he insisted that scholars should give the exact titles and editions of the works they cited, he often gave incomplete bibliographical details in his own references. He regularly found himself forced to cite books from memory or from notes that he could not verify. Worse still, he cited sources that he had not read at all, drawing his information from summaries and reviews. But the novelty and utility of the model he offered are now clear.

So is the stimulus that Bayle offered to young intellectuals who wished to preserve the possibility of attaining historical knowledge while developing a critical and modern epistemology and practice. Writers on the credibility of historical testimony (*de fide historica*) like the German F.W. Bierling followed Bayle's hints as they explicitly addressed the wider problem of establishing rules for the criticism of sources. Long before Ranke made archive diving fashionable, Bierling had pointed out (in a book festooned with footnotes) that archives can mislead. He admitted that many of his contemporaries thought this impossible, but careful analysis, he argued, proved his point. Archives consisted chiefly of documents created by ambassadors and other public officials. But such men normally had to report on deliberations to which they did not have direct access and on the intentions of

FOTO MARBURG/ART RESOURCE

G. W. LEIBNIZ

monarchs who did not speak frankly. Their reports, in short, contained "what the ambassador guesses to be true or considers to be memorable, not always what is true."

A neat footnote drove Bierling's point home: Hugo Grotius, while serving as ambassador in the Swedish service, spent the whole day and much of the night writing theology and satisfied his employer, the statesman Axel Oxenstierna, with the gossip he picked up in the streets ("des nouvelles du Pont-neuf en beau latin"). An archive constituted of such reports—and a narrative derived from them—might yield the right names and dates but would hardly provide the inner history of events. Moreover, archives and narratives kept and compiled in good faith contradicted one another. Bierling did not take this as reason to despair; rather, he argued coherently for a mitigated, rather than excessive, faith in historical research. He, in turn, was only one of the best known among the many writers who took part in the sophisticated debates of the late

3 ONE CURÉ DISPARAGED THE DISPLAY OF MERE "BOOKSELLER'S LEARNING."

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seventeenth and eighteenth centuries on the reasons for historical skepticism and the conditions of historical credibility. These writers, as Markus Völkel has shown, did not always arrive at new results and rigorous formulations. But they highlighted both the problems connected with establishing historical facts and the accomplishments of scholars who had attacked particular problems, like the dating of manuscripts.

NATURALLY, it took time for anything resembling a uniform citation practice to establish itself in the varied ecologies where Europe's scholars fought with note and claw for intellectual space. Even within a single province of the Republic of Letters, moreover, one writer's citation practices often provided ammunition for another's polemical broadsides. When the erudite curé Jean-Baptiste Thiers set out to excoriate Jacques Boileau's critical history of the place of flagellation within the Christian tradition, he flailed his opponent, who had denied the antiquity of the practice, for swelling his book by including irrelevant details about his sources: "Often he cites the year and place of publication of books, the names of the printers or publishers, the pages and leaves in the books, and sometimes even the capital letters found at the margins and the lines on the pages." Boileau, Thiers complained, filled his work with such unnecessary bibliographical facts, such "bookseller's learning," even when only one edition of the work in question existed. Sometimes Boileau reached a level of pedantry not seen in the whole period since the invention of printing. Yet in other cases he omitted all details. "What purpose," Thiers demanded, "do all these meticulous and affected citations serve, except to enlarge his history?" Even in the erudite precincts of the French clergy, evidently, too much learning could prove a passport to dismissal as the social inferior of those who wore their documentation more lightly.

In the course of the later seventeenth and eighteenth centuries, however, a long series of debates and discussions among writers, translators, and printers gradually yielded something like the modern system of documentation—even if the process did not then reach, and still has not reached, completion. Across

Improvisations on a Missing String

Nazek Saba Yared

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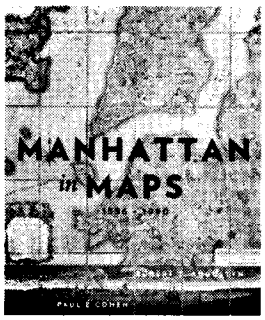

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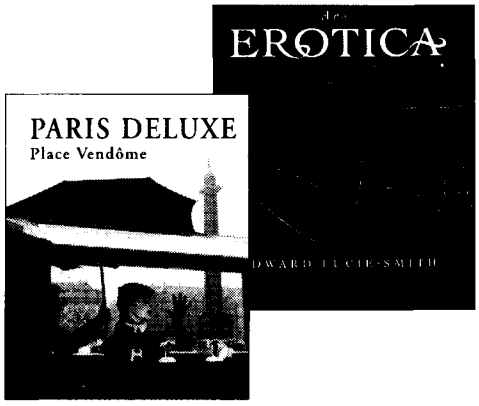

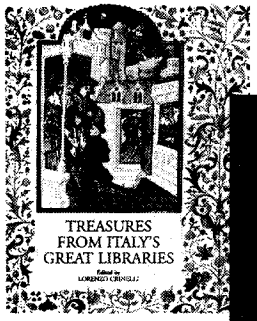

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Europe, writers and publishers collaborated more intensively than ever before, trying to make every aspect of the physical presentation of text guide the reader through its content. A revolution in book design took place, as those concerned with authorship and publication carried out experiments in layout and design, trying to make books physically as well as intellectually accessible. In this period, for example, classical scholars and printers first collaborated to establish the custom that the lines of each book or section of a classical text be numbered sequentially throughout. Thus critics all over Europe could discuss a common problem without assuming that all participants knew the texts by heart or having to refer to pages and lines in a single edition—practices that had been standard since the invention of printing.

The combination of practical and aesthetic considerations that moved the classicists to depart from immemorial procedures also affected historical practice. As footnotes became not only intellectually fashionable but also typographically practical, they came to be found in the historian's normal literary toolbox. Through the eighteenth century, new standards for precision gradually infected historical exposition. Historians continued to believe in the moral and literary virtue of a clear, instructive narrative but also cherished a newer desire for critical discussion of sources. Publishers needed to reach large markets but also wanted to work with authors. An interminable struggle resulted, one sometimes fought out to the tune of "two steps forward, one step back." But in the end, thanks as much to wider developments in publishing and education as to the achievements of brilliant individuals, the footnote won its place on the historian's page.

A letter written by David Hume in 1776 offers crucial testimony to the footnote's acceptance. Wishing to insist that Gibbon make his endnotes into footnotes, Hume directed his correspondence not to Gibbon himself but to their joint publisher, William Strahan. As he wrote, "I intended to have given him [Gibbon] my Advice with regard to the manner of printing it; but as I am now writing to you, it is the same thing." Hume's new sense of how history should be read went together with a new sense of how it should be written—and that, in turn, with a new sense of what the author could expect of his publisher. For all of this, he owed a considerable debt to the French thinker of the late seventeenth century who found in Holland a refuge from the religious intolerance of Louis XIV, in learning a refuge from the oppression of theological orthodoxies, and in footnotes a refuge from the intellectual dogmatism of Descartes.

Anthony Grafton is Dodge Professor of History at Princeton University. He is the author of *Joseph Scaliger* (Oxford), *Defenders of the Text*, and *New Worlds, Ancient Texts* (both from Harvard).

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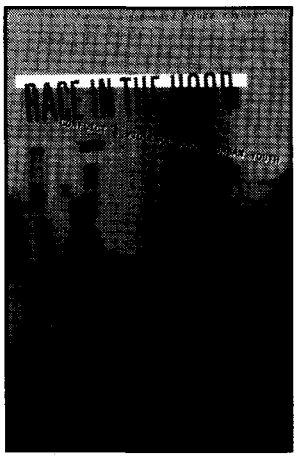

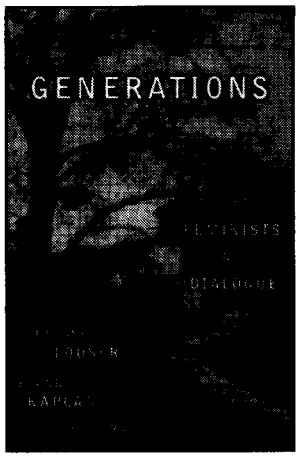

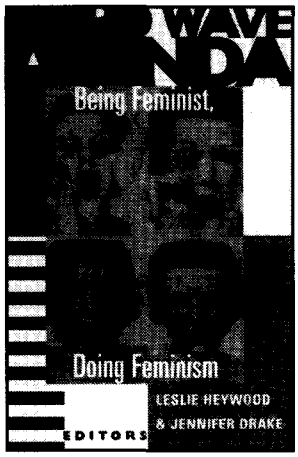

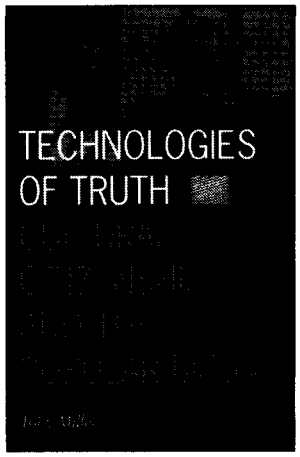

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catalog will reveal that this is hardly the case. This fall, we're publishing Jimmy Carter's spiritual reflections on the Bible and Donald Trump's account of his comeback from the abyss of bankruptcy. Next year, Times Books will publish Sears CEO Arthur Martinez's story of his company's stunning recovery and former Hong Kong governor Chris Patten's speculations on Asia's future, among many other works of nonfiction.

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THE SOUND OF SILENCE

I was saddened but not really surprised to learn from Adam Shatz ["The American Earthquake," *LF*, September] that Mike Davis's *City of Quartz* has not received much response from specialists in urban geography. Though academics often profess a desire to reach a larger audience, they typically maintain a professional silence when confronted with a book that has actually done so. They may talk about the book privately, but they are not likely to write about it in professional journals.

This is where the generalist reviewing media play their most important role in the intellectual life of the country, for if specialist silence is harmful, *total* silence is even more harmful, not least for the specialists themselves. *City of Quartz* was widely reviewed in newspapers and won the 1991 nonfiction prize of the National Book Critics Circle, most of whose members are newspaper reviewers and review editors. Sometimes journalism reports scholarship. Other times—and this may be one—it rescues it.

JACK MILES
MELLON VISITING PROFESSOR OF HUMANITIES
CALIFORNIA INSTITUTE OF TECHNOLOGY
PRESIDENT, NATIONAL BOOK CRITICS CIRCLE, 1990–1992

LOBSTER TALE

I read the article concerning the debate about T.S. Eliot's lines "I should have been a pair of ragged claws/Scuttling across the floors of silent seas" ["Shell Game," *LF*, September] with complete astonishment. Some of the finest minds in literary criticism and scholarship discuss the lines as if they had something—anything—to do with crabs or lobsters.

You don't have to be William Jennings Bryan at the Scopes trial (or, for that matter, Jesse Helms) to recognize a direct reference to Darwinian evolution used as

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poetic metaphor for Prufrock's angst.

Having finished English literature grad school many years ago—and never set foot in a classroom again—I'm almost embarrassed (really delighted) to explain the lines to those who think in terms of boiled-and-broiled versus what's on toast. The "ragged claws," barely organic, hardly sentient, move blindly across the bottom of the Paleozoic ooze to emerge eons later, beginning the ascent to the creature in the rich and modest necktie who is overcome by Weltschmerz.

Each of these academics deserves an F—but I'm willing to raise it to a D minus because all of them have done good work in the past.

JACK SAWYER HARRIS
NEW YORK, NY

PHOTO SYNTHESIS

In Adam Shatz's article, "Strange Fruit" [*LF*, August], I am quoted as saying that *The Journal of American History* "deliberately played up a false dichotomy between blacks and whites" in publishing photographs alongside the essays of Joel Williamson's peer reviewers. My point was that many *readers* I spoke with believed that the author photographs were intentionally used to focus attention on racial differences when, in fact, this effect was purely coincidental. My other point was that too many readers treated the differences in the readers' reports as reflective merely of racial identity rather than of differences in political and intellectual perspectives—a point on which Shatz also concurs.

ROBIN KELLEY
DEPARTMENT OF HISTORY
NEW YORK UNIVERSITY

BAD SEED?

In "Rain Men" [*LF*, August] Linda Baker writes that "In Salt Lake City, TRC North American Weather Consultants promises rainfall increases of 25 percent." In a telephone interview, I specifically told Baker that we do not guarantee any particular results. Rather, our anticipation is a 5 to 25 percent increase depending on the types of clouds present during the operational period.

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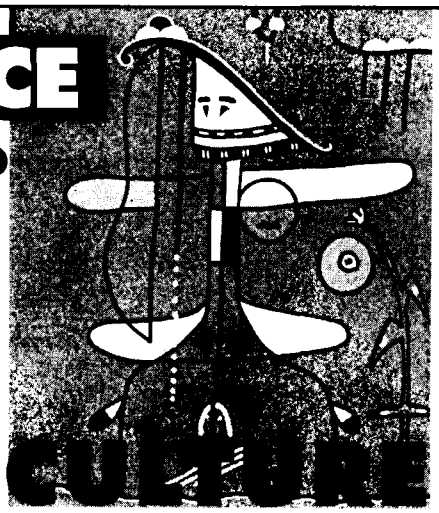
CORRECTION

In "Nightmare on Twelfth Street" [*LF*, August], *Lingua Franca* erroneously reported that Adamantia Pollis won her sex-discrimination suit against the New School for Social Research on the grounds that she was forced to retire at age seventy while several male contemporaries were permitted to continue teaching. In fact, the grounds of her suit was wage inequity between male and female employees.



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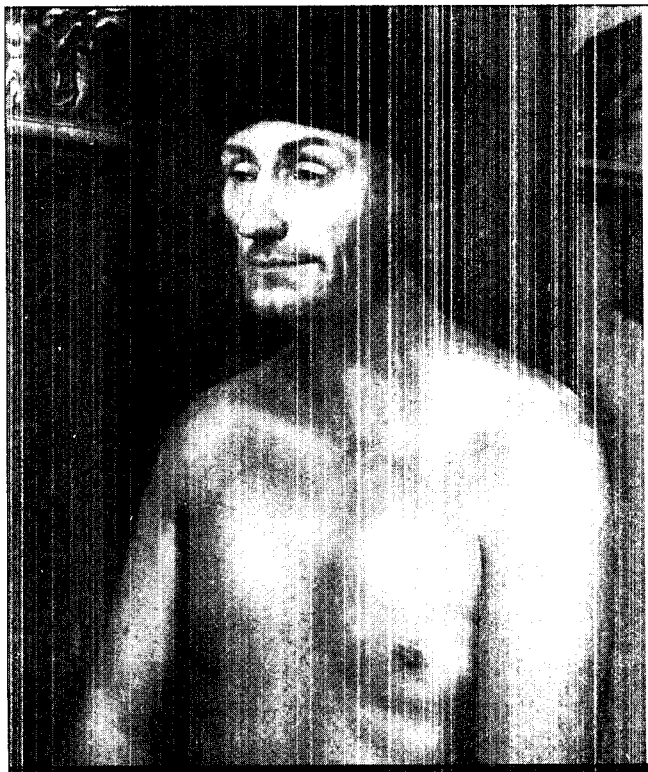

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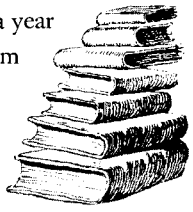

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Hyphotheses

THE MONSTER

and other mathematical beasts

FOR A DISCIPLINE THAT IS supposed to yield certain knowledge, mathematics is awfully uncertain about what its objects are. The ancient Egyptians took a no-nonsense approach: Their geometry was about stretched ropes and plots of land near the Nile. But when the Greeks got hold of mathematics, they started loading it with metaphysical baggage. The Pythagoreans worshiped numbers as a divine gift and made them the foundation of reality. Plato took this a step further, enshrining mathematical forms like numbers and circles in a kind of eternal, heavenly realm, one that revealed itself to the intellect but not to the senses.

Scratch a working mathematician today and you're likely to find a Platonist underneath. The things described by mathematics—not just circles and numbers but more rococo entities like Galois fields and Gorenstein rings—are widely believed within the profession to exist objectively and eternally like Plato's ideas, transcending the world of space and time. René Thom, the French geometer who became famous in the 1970s as the father of catastrophe theory, has declared that "mathematicians should have the courage of their most profound convictions and thus affirm that mathematical forms indeed have an existence that is independent of the mind considering them." Mathematical abstractions are "like Mount Everest," says Oxford's Roger Penrose. They are "just there."

Other mathematicians are loath to swallow this metaphysical bolus. The distinguished University of Chicago algebraist Saunders MacLane, for one, dismisses the Platonist vision of mathematics as "a glorious illusion." British physicist David Deutsch, in his recent book *The Fabric of Reality* (Penguin), calls it a "rickety fantasy."

But what is the alternative? Consider, if you will, the Monster. This is an entity—a "group"—that mathematicians discovered in 1980, on their way to wrapping up a bit of unfinished business in abstract algebra. The concept of a group is one of the more abstract (and powerful) in higher mathematics. Speaking crudely, a group is a set of operations—operations on anything at all—that hang together in a nice algebraic way. In the case of the Monster, the operations are ways of shifting things around in higher-dimensional space—in 196,883 dimensions, that is.

The Monster is vast: The number of its elements can be written down roughly as 8 followed by 53 zeros. Where might the beast live, if not in Platonic heaven? Could it really be confined to the heads of a few mathematicians, like a collective neurosis? Could it lurk somewhere in the physical universe, like a black hole? Might it be just a fiction, like Prince Hamlet? A sociocultural construct, like the national debt? An unactualized possibility, like the fortune you could have made had you gone into software ten years ago?

Strange to say, all of these ontological hypotheses have their partisans today. While discord in the mathematical priesthood is nothing new—in the 1920s proponents of various alternatives to Platonism were persecuting one another with all the fury of early Christian heresiarchs—the debate over what mathematics really is has never been more bewildering.

Among those rendered irritable by all the philosophical confusion is Reuben Hersh, a retired mathematician whose book *The Mathematical Experience* (co-authored with Philip J. Davis), won the National Book Award in 1983. In his just-published *What Is Mathematics, Really?* (Oxford), Hersh has a great deal of fun saying boo to the goose that honks Platonism. "Forget foundations, forget immaterial, inhuman 'reality,'" he exhorts. Mathematics is about "a world of ideas...created by human beings, existing in their shared consciousness." He calls this doctrine humanism. Moreover, he says, most of the thinkers who have shared his understanding of mathematics—Aristotle, d'Alembert, Polányi—have been "lefties" (good), whereas the Platonists and other foundationists have been "righties" (bad). Gottlob Frege died a Nazi, and Willard Quine is an avowed Republican!

One problem with Hersh's humanism: Ideas in the head, whether shared or unshared, do not possess the features we want to ascribe to mathematical objects. The Monster has 8 zillion zillion parts; no amount of Jamesian introspection will reveal a like multiplicity in one's *idea* of the Monster. Fine, Hersh concludes, then the Monster does not really exist—even though different mathematicians can reason about it and reach the same conclusions as to its attributes, which thereby receive "social validation."

Will mathematicians rally to Hersh's fashionable pragmatism? I hope not, for the deepest and best mathematics always seems to be done by Platonists. Even if Platonism is false, it is probably most effective for working mathematicians to behave as though it were true. Wait, doesn't that *make* it true from a pragmatic perspective?

It should be borne in mind, though, that too ardent a belief in mathematical Platonism can bring one to a sticky end. In the nineteenth century, Georg Cantor framed modern set theory, discovering in the bargain a never-ending hierarchy of infinite sets—a vision he believed was vouchsafed to him by God. He ended his days in an insane asylum. Arch-Platonist Kurt Gödel claimed to have an extrasensory perception of mathematical entities. He starved himself in 1978 in Princeton (where he was a member of the Institute for Advanced Study), under the paranoid delusion that people were putting poison in his food.

Platonic relationships, it seems, can be dangerous to your mental health.

JIM HOLT

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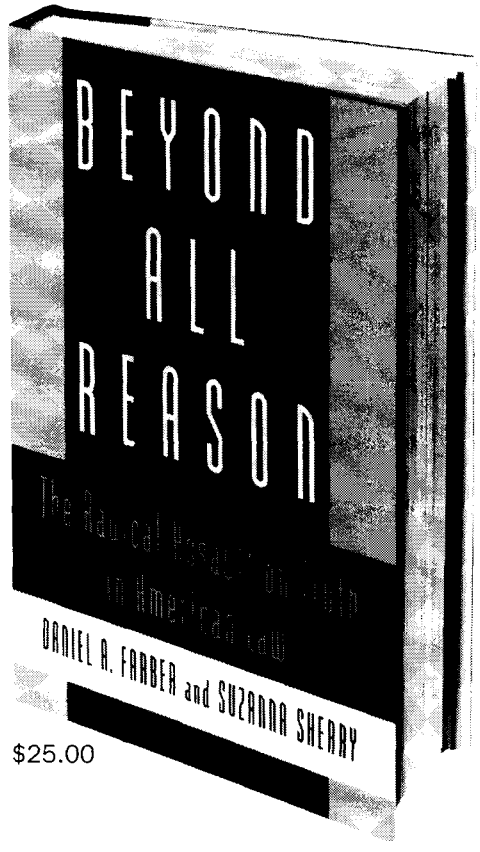

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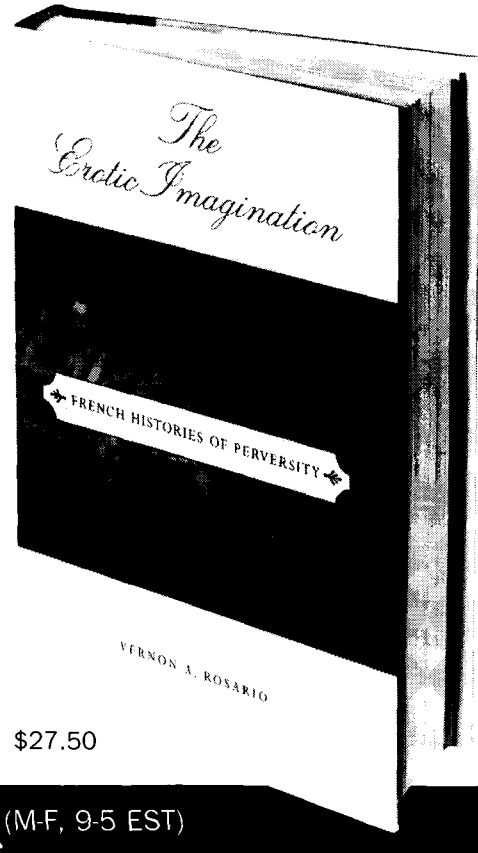

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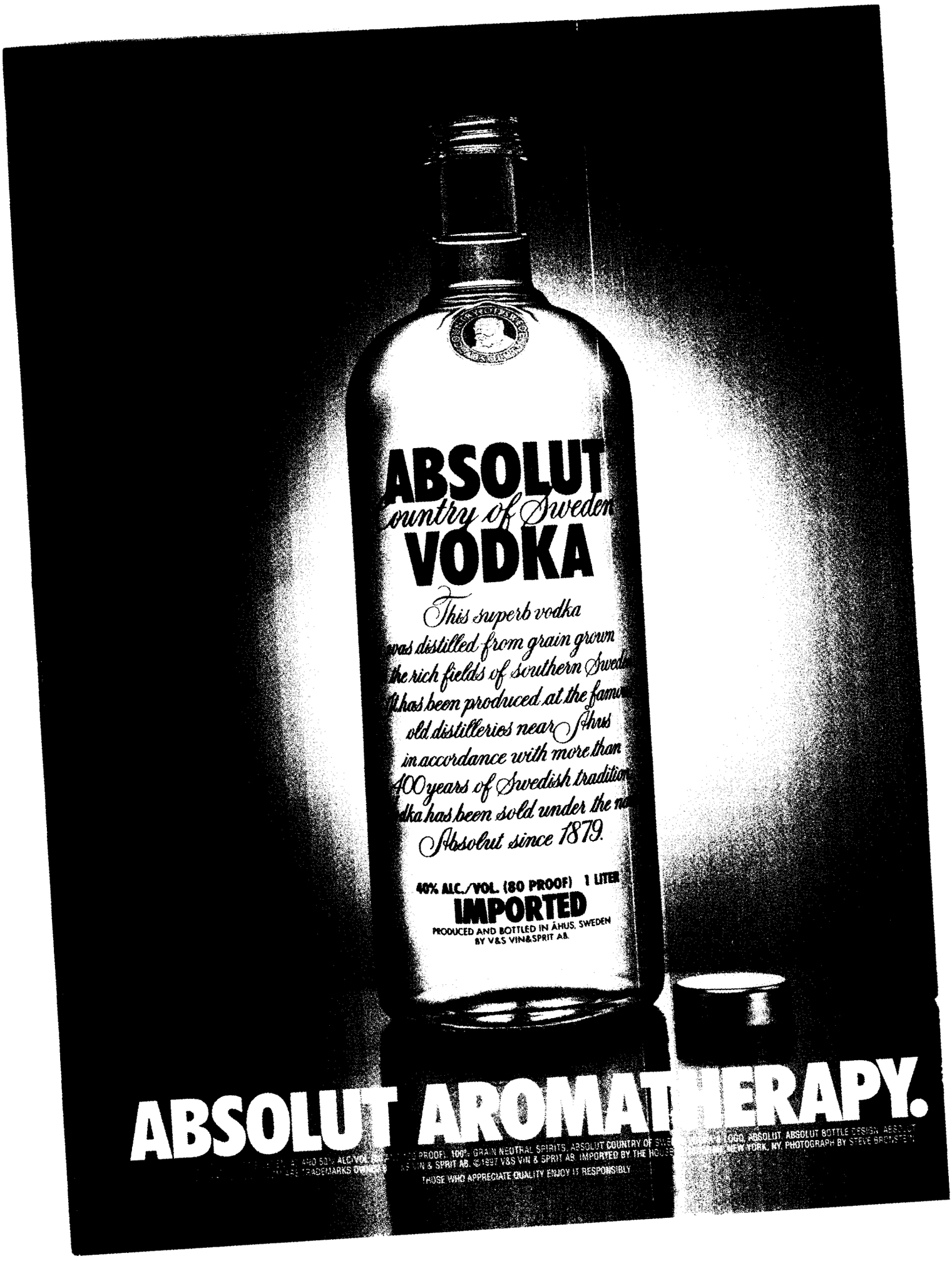
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